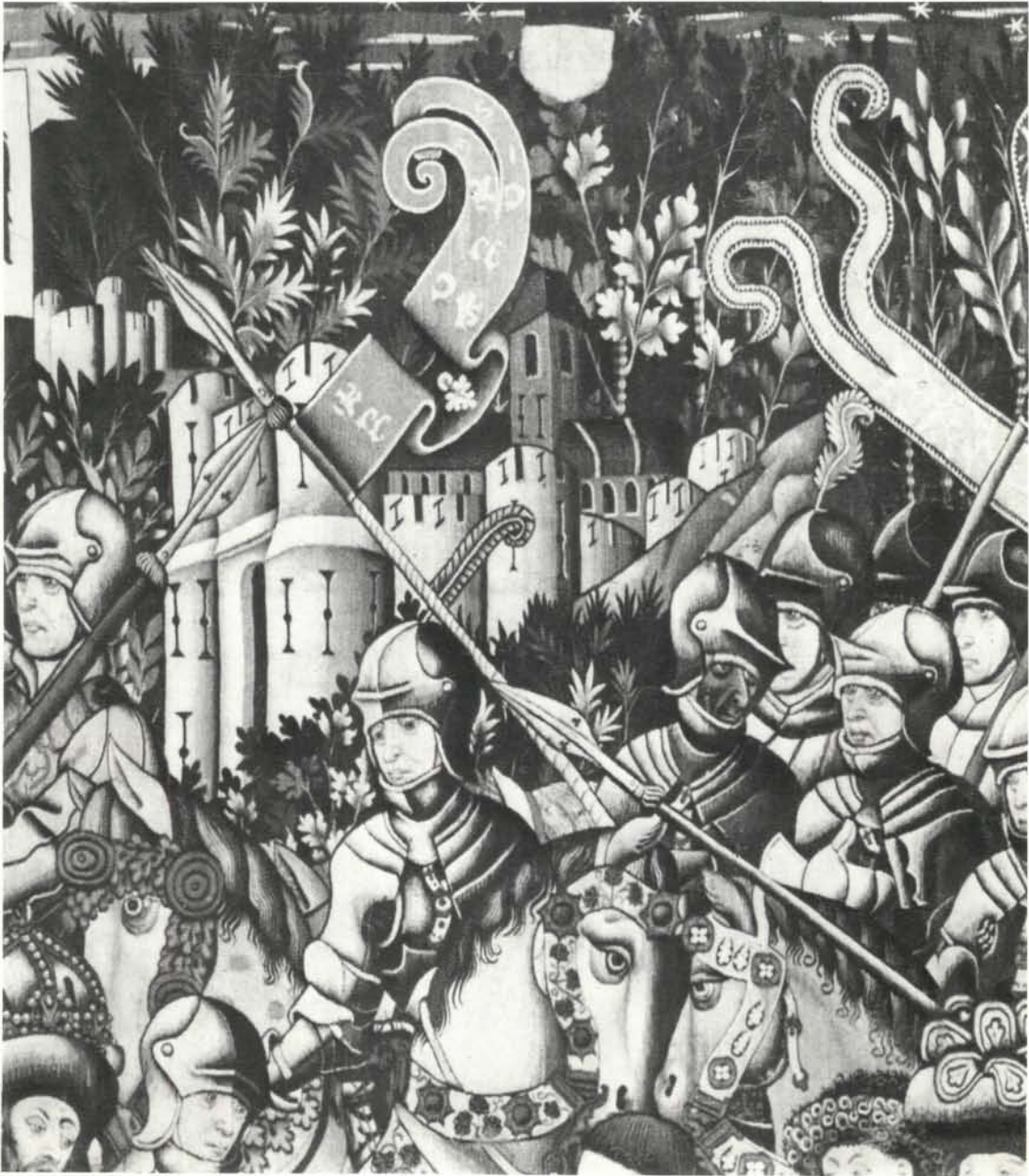


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The Rise of Administrative Kingship: Henry I and Philip Augustus

C. WARREN HOLLISTER
AND
JOHN W. BALDWIN

MODERN HISTORIANS HAVE LONG RECOGNIZED the importance of the reigns of Henry I (1100–35) and Philip Augustus (1179–1223) as *tournants* in the respective national histories of England and France. Although separated by nearly a century, the two kings met similar problems with similar solutions. In 1105–06 Henry conquered Normandy and rejoined it to the realm of England; in 1203–04 Philip seized this same wealthy province, along with others, and joined them to the realm of France. Ruling their expanded dominions for many years thereafter, both kings faced hostile coalitions and invasions designed to undo the effects of the conquests. For purposes of defense and political stability the two kings not only adapted existing governmental machinery to new purposes but, more importantly, created new administrative institutions to increase the effectiveness of their rule. These innovations—largely unperceived by their contemporaries—gestated during the long reigns of Henry and Philip. Only during the subsequent and more celebrated reigns of their grandsons, Henry II and Louis IX, were the full consequences of these achievements realized.

Although the accomplishments of Henry I and Philip Augustus are well known to the historians of medieval England and France, their parallel roles in introducing administrative innovations into their lands invite comparison. Rather than seek the inspiration for these changes—whether it be Anglo-Saxon, Frankish, Norman, Flemish, or whatever—or dwell on accidental similarities—longevity, personality, good fortune, and the like—that condition all historical phenomena, we shall probe the common underlying institutional structures of England and France to understand the significance of the

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changes that took place during both reigns. This requires two stages of analysis: first, to identify the regime that each king inherited from his predecessors and, second, to seek out the innovative changes that followed. Such an analysis makes it clear that comparable problems stimulated the two kings to respond in comparable ways.

Both Henry I and Philip Augustus received from their forebears regimes founded on two essential features: an ambulatory central court and fixed local officials. This system functioned effectively because the relatively small size of the royal dominions permitted the itinerant royal court to keep in contact with local officers. Although Henry and Philip perpetuated this traditional arrangement, they added to their administrative inheritance four significant innovations designed to meet the challenges posed by enlarged realms. First, viceregencies were developed to bear the increased burdens placed on the kings themselves: in Henry's case, to serve in his absence on the opposite side of the Channel; in Philip's, to provide the king with a more efficient lieutenant. Second, exchequers or central accounting procedures were fashioned to tighten curial control over the treasuries of both realms and monitor local officials more closely. Third, as governmental business became more complex, improved records were maintained and stored to establish continuity of royal policy. And, fourth, traveling agents were dispatched from the central court to expand the scope of royal justice and to increase the central government's contact with its subjects. By these four devices the English and French kings created the administrative machinery necessary to their fundamental policies of centralization and stabilization.

The institutional innovations shared by Henry and Philip were not, however, sufficient in themselves to meet the increased demands of government. Like "developing" nations of the present, medieval regimes were hampered by a lack of loyal, properly trained personnel sufficient to operate the new administrative machinery. Ultimately, the recruitment of qualified and trusted agents was the leading problem confronting medieval governments. Although Henry I and Philip Augustus inherited similar administrative practices and expanded them in similar ways, they differed sharply in their recruitment and employment of personnel, and a close analysis of the sorts of people that Henry and Philip drew into their administrations discloses a previously unsuspected contrast between the two monarchies.

HENRY I

HENRY I, THE YOUNGEST AND ABLEST of William the Conqueror's sons, built an administrative system extraordinary for its day, more effective and, to some, more oppressive than any government the transalpine West had known since the time of the Roman Empire. To a remarkable degree, Henry's government

kept the peace in England and Normandy alike.¹ Albert Brackmann has described it as “a new type of political organization” that “set the development of European civilization on a new course.”² But despite its precociousness, Henry’s regime was not the total novelty that Brackmann’s enthusiastic phrases suggest. The king and his administrators were as inclined to adapt as to invent, and it is not always easy to distinguish their innovations from their adaptations.

As a son of William the Conqueror, Henry drew on the strong political traditions of both the Norman dukes and the Anglo-Saxon kings. The authority of the pre-Conquest duke of Normandy had surpassed that of all other French princes, including the king of France. The ducal court moved constantly about the duchy dispensing justice, issuing charters, supervising the collection of revenues and the minting of coins. Ducal kinsmen served as counts of the major frontier districts—Mortain, Eu, Évreux—and Duke William commanded their direct obedience as well as that of his regional officials, the vicomtes.³ The Anglo-Saxon royal administration, which William had seized and perpetuated, was more effective still. As ruler of England the Conqueror inherited a well-articulated system of shires and sheriffs, shire courts and hundred courts, and at the center an ambulatory royal entourage that exercised an unusual degree of control over the kingdom’s military and fiscal resources. William gained control of a royal chancery which had developed that potent instrument of monarchical authority, the sealed writ. He likewise took possession of the royal treasury at Winchester, along with a revenue system well designed to keep it filled. The system included set sheriffs’ farms, a tight grip on the kingdom’s mints and minters, and a unique property tax—the danegeld—collected through a comprehensive assessment network of hundreds and wapentakes, hides and carucates.

In the years after 1066 a tenurial revolution swept away the Old English landed aristocracy and replaced it with a northern French—largely Norman—aristocracy. The English lands of this new aristocracy usually consisted not of compact territorial blocs but of manorial clusters, large and small, scattered across a number of shires. The Conqueror’s half-brother Robert, count of Mortain, for example, held lands in twenty shires and Hugh, earl of Chester, in nineteen. This dispersion of great estates, including the king’s own, was of fundamental importance to the growth of royal government. As Sir Richard Southern has explained, “It was tenurial complexity that gave royal officials their opportunities, by making all free tenants more or less

¹ C. Warren Hollister and Thomas K. Keefe, “The Making of the Angevin Empire,” *Journal of British Studies*, 12 (1973): 4–5. William of Malmesbury credited Henry with giving Normandy “a peace such as no age remembers, such as his father himself . . . was never able to effect”; William of Malmesbury, *Gesta Regum Anglorum*, ed. William Stubbs, Rolls Series, no. 90, vol. 2 (London, 1889): 476.

² Brackmann, “The Beginnings of the National State in Medieval Germany and the Norman Monarchies,” in Geoffrey Barraclough, ed. and trans., *Medieval Germany, 911–1250: Essays by German Historians* (Oxford, 1948), 287–88.

³ See C. Warren Hollister, “Normandy, France, and the Anglo-Norman Regnum,” *Speculum*, 51 (1976): 205, references cited therein; and R. C. Van Caenegem, ed., *Royal Writs in England from the Conquest to Glanvill: Studies in the Early History of the Common Law*, Selden Society, no. 77 (London, 1959), 57–58.

equal in the royal courts, and inducing all men, however great, to acquiesce in the growth of royal justice."⁴

As a consequence of the dispersion of great estates and of administrative tightening under William the Conqueror and William Rufus, Henry I inherited in 1100 a government considerably more effective than it had been in 1066. The king's control of litigation had grown appreciably through the practice of sending teams of royal justices on *ad hoc* commissions to settle important disputes and the appointment of local justiciars to handle pleas of the crown in the shires and hundreds. Royal revenues were much higher than before, thanks to the doubling of the king's demesne lands during the post-Conquest settlement, the mass of data collected in the Domesday survey, and the fiscal chicanery of Rufus's chief minister, Ranulf Flambard. Immediately after his accession Henry I put the unpopular Flambard into the Tower of London as a highly visible act of public relations. More significantly, however, Henry won the support of virtually all of Rufus's remaining administrators and kept them in office alongside new men from his own entourage.⁵ He had no intention of dismissing Rufus's officials and starting from scratch.

THE DOMINATING THEME OF HENRY I'S GOVERNMENT was centralization. Only later would household offices begin to drift out of the itinerant court and settle down as departments. Henry's reign saw the converse trend toward ever-tightening control by the *curia regis*. Both Southern and the collaborators Henry G. Richardson and George O. Sayles refer to Henry's administration as a "machine";⁶ if so, it was a machine powered by a single piston.

The king's court, incessantly moving across England and Normandy, was the chief source of royal justice, governance, and patronage. Its membership fluctuated constantly. Some were almost always in attendance, others frequently so, and others occasionally or rarely. Besides officials and servants of the royal household, the court included a less structured group of royal advisers and *familiars*. It also might include visiting foreign princes, royal and comital heirs-apparent and bastards with their tutors and servants, camp followers, and plaintiffs and favor-seekers from the neighborhoods through which the court traveled or, sometimes, from afar.

Until early in Henry's reign, the Anglo-Norman *curia regis* was a disorganized, predatory mob. Eadmer of Canterbury told chilling tales of Rufus's entourage plundering and destroying the countryside, laying waste to

⁴ Southern, *Medieval Humanism and Other Studies* (Oxford, 1970), 229. This fragmentation of holdings probably resulted both from estates being granted at various times during the course of the post-1066 consolidation and from pre-Conquest land patterns often being similarly dispersed, though to a lesser degree. See Reginald V. Lennard, *Rural England, 1086-1135* (Oxford, 1959), 28-39.

⁵ C. Warren Hollister, "The Anglo-Norman Civil War: 1101," *English Historical Review* [hereafter *EHR*], 88 (1973): 318-19, and "Magnates and *Curiales* in Early Norman England," *Viator*, 8 (1977): 63-81.

⁶ Southern, *Medieval Humanism*, 210; and Richardson and Sayles, *The Governance of Mediaeval England from the Conquest to Magna Carta* (Edinburgh, 1963), 163, *passim*.

all of the lands through which the king passed, taking indecent liberties with local wives and daughters, getting drunk on stolen wine and, when they could drink no more, washing their horses' feet with it and pouring the rest on the ground.⁷ Henry I put an end to all this. He issued strict regulations limiting the practice of requisitioning and establishing fixed prices for local purchases. He also established specific offices and allowances for his household and stipends for magnates in attendance, arranging that everyone in his retinue should receive set payments for their subsistence.⁸

At the heart of the *curia* was the royal household staff, whose organization and fixed allowances were recorded in detail in the *Constitutio Domus Regis* of circa 1136. The chief household officers, echoing Continental custom, were the chancellor, stewards, master butler, master chamberlain, and constables. To these traditional ministers Henry added a new one—the household treasurer. Scholars used to dispute whether the *Constitutio Domus Regis* described the English household or the Norman, but they now generally agree that the *domus regis* accompanied the king wherever he might be.⁹ The frequency with which Henry's household officers attested royal or administrative *acta* emanating from Normandy and from England supports this conclusion, at least to a degree. When due weight is given to the survival of Henry's English charters in far greater numbers than his Norman charters, it becomes clear that most of Henry's household officials regularly crossed with the *curia* and attested with some frequency on both sides of the Channel (see Table 1). But certain household officials concentrated their activities on one side of the Channel or the other. The steward Robert de la Haye attested a great many royal acts in Normandy but very few in England, and the same is true of the chamberlains William fitz Odo and William of Glastonbury and, to a lesser extent, the steward Robert of Courcy. Conversely, certain household officers attested charters primarily from England: the steward Adam of Port and the constables Walter of Beauchamp, Walter and Miles of Gloucester, and Nigel and Robert of Oilli.¹⁰

⁷ Eadmer, *Historia Novorum in Anglia*, ed. Martin Rule, Rolls Series, no. 81 (London, 1884), 192–93. Also see the *Anglo-Saxon Chronicle*, A.D. 1097 (Rufus), 1101 (Robert Curthose), and 1104 (early Henry I).

⁸ Eadmer, *Historia Novorum in Anglia*, 193; Malmesbury, *Gesta Regum Anglorum*, 487; and Walter Map, *De Nugis Curialium*, ed. M. R. James (Oxford, 1914), 219, 235.

⁹ For the *Constitutio Domus Regis*, see Charles Johnson, ed., *Dialogus de Scaccario* (London, 1950), 129–35. And see G. H. White, "The Household of the Norman Kings," *Transactions of the Royal Historical Society*, 4th ser., 30 (1948): 127–59; H. W. C. Davis et al., eds., *Regesta Regum Anglo-Normannorum*, 4 vols. (Oxford, 1913–69), 2: ix–xvii; and, most recently, John Le Patourel, *The Norman Empire* (Oxford, 1976), 135–37.

¹⁰ The responsibilities of Walter and Miles of Gloucester (father and son) included the custody of Gloucester Castle. Walter of Beauchamp's constablership seems to have been associated with Worcester Castle, and Nigel and Robert of Oilli (father and son) were responsible for Oxford Castle; *Regesta Regum Anglo-Normannorum*, 2: xv–xvi. Robert of Oilli, although he attested heavily in England, was rather more active in Normandy than his father had been. Between Henry's conquest of Normandy in 1106 and his death in 1135, he spent about 210 months in Normandy and 140 in England; Le Patourel, *Norman Empire*, 124, 175–76. But, of approximately 980 charters issued during these years, in which the place of origin can be definitely or probably determined, about 765 emanated from England and only about 215 from Normandy. This information, and much that follows, has been drawn from comprehensive (but as yet unpublished) tables prepared by Brian A. Foster that measure the English and Norman attestations of all of Henry I's *curiales*. Foster's study of attestations and crossings will, when completed, provide a far more rigorous analysis of the problem than has been possible here.

TABLE 1
Officers of the Royal Household, 1130

<i>Office</i>	<i>Officers of the Household</i>
Chancellor	Geoffrey Rufus* [XE]
Keeper of the Seal	Robert <i>de Sigillo</i> * [X]
Stewards	Hugh Bigod* [X] Humphrey of Bohun* [X] Robert of Courcy [XN] ^a Robert de la Haye* [N] (?) William Martel [X] (?) Robert fitz Richard (Clare) [X]
Master Butler	William of Aubigny <i>Pincerna</i> * [X]
Treasurer	Nigel <i>nepos episcopi</i> * (bp. of Ely, 1133 ff.) [X]
Chamberlains of the Winchester Treasury	William of Pont de l'Arche* [X] Geoffrey of Clinton* [X]
Chamberlains of the Camera Curiae	William of Pont de l'Arche* [X] Osbert of Pont de l'Arche [X (?)] William Mauduit [X]
Chamberlain of England and Normandy	(?) Rabel of Tancarville [N] ^b
Norman Chamberlains (?)	William fitz Odo [N] ^c William of Glastonbury [N]
Constables	Robert de Vere* [X] Miles of Gloucester* [E] Robert of Oilli [XE] Walter of Beauchamp [E] Brian fitz Count* (attested as constable in 1131) [XN]
Assistant Constables(?)	Roger of Oilli [E] Henry de la Pommeraie [X]
Marshals	John fitz Gilbert [X] Wigan the Marshal [N (?)]

NOTE: An asterisk (*) designates a *curialis*; for a definition of a *curialis*, see page 887, below; and, for Henry's *curiales*, see Table 4, below. In their roles as attestors of royal acts, E = 91–100 percent English attestations; XE = 81–90 percent English; X = 51–80 percent English; XN = 50–64 percent Norman; and N = 65–100 percent Norman. (Some adjustment has been made for the greater survival expectancy of charters from England; pre-1106 charters have been excluded from all computations regarding English versus Norman attestations.)

^a Robert of Courcy attested thirty-seven acts between 1113 and 1135 (1.7 per year), but thirty-five of those attestations occurred between 1126 and 1135 (4.1 per year).

^b Rabel “inherited” the Anglo-Norman chamberlainship from his father, William of Tancarville* [X], who died in 1129. But there is no concrete evidence that Rabel performed any functions of the chamberlainship under Henry I. In 1133 the *curialis* Aubrey de Vere received the master chamberlainship “of all England” but he was at times with Henry in Normandy between 1133 and 1135; see *Regesta Regum Anglo-Normannorum*, 2: nos. 1777, 1913–15, 1960.

^c William fitz Odo averaged 1.9 attestations per year, which places him about thirty-second among attestors active in 1130. But the disproportionate loss of Norman acts causes a systematic underestimation of the curial activity of primarily Norman attestors. See Table 4, below.

This same kind of attestation analysis can help to illuminate the administrative activities of Henry's chancellors: Ranulf (1107–22) and Geoffrey Rufus (1123–33) both attest enormous numbers of surviving royal acts, some from Normandy but far more from England.¹¹ When Henry was in Normandy, his chancellor was often away from the itinerant court, engaged in administrative work in England. And, since the Anglo-Norman chancery was inseparable from the royal court, the household *scriptorium* must have often functioned without the chancellor's direct supervision. During the latter half of Henry's reign, the chancery staff included about four scribes, under the direction of a subchancellor known as the *magister scriptorii* or keeper of the king's seal.¹² Robert *de Sigillo* occupied this office from 1121 to 1135, and his nearly one hundred attestations, nicely balanced between England and Normandy, suggest that he was regularly with the king's court. The frequency and ease with which the chancery functioned without the chancellor's presence doubtless influenced Henry's decision to leave the chancellorship vacant after Geoffrey Rufus's promotion to the bishopric of Durham in 1133 and may also explain why Henry was willing to double Robert *de Sigillo*'s wages.¹³

That an important minority of royal household officials concentrated their activities on only one side of the Channel is not surprising. Once he had rejoined the duchy to the kingdom, Henry had to depend on some sort of regional governing body to supervise Normandy when he was in England and to administer England when he was in Normandy. His predecessors, William I and William II, faced with the same problem of dual governance, had handled it with a variety of *ad hoc* arrangements. William I left the kingdom in the charge of various great men at different times—men like William fitz Osbern; Odo, bishop of Bayeux; and Archbishop Lanfranc. The Conqueror likewise entrusted Normandy to such notables as Queen Matilda, Roger of Montgomery, and Roger, lord of Beaumont. Under William Rufus, whose joint rule of Normandy and England only extended from 1096 to 1100, the first hint of a more specialized English viceregency appeared. Initially, William II's omnicompetent royal minister, Ranulf Flambard, served as coregent with Walchelin, bishop of Winchester; but in 1099 Rufus committed England to Flambard alone, to be assisted by two administrative lieutenants of only moderate wealth—Hamo the Steward, sheriff of Kent, and Urse of Abitôt, sheriff of Worcestershire.¹⁴

¹¹ Ranulf attested 163 charters, of which 134 are certainly or probably from England and 22 certainly or probably from Normandy; of Geoffrey Rufus's 115 attestations, about 95 are English and about 15 are Norman (a few are of undetermined origin). Even allowing for the much greater survival rate of English charters, it remains clear that both men were often in England while their king was in Normandy.

¹² T. A. M. Bishop, *Scriptores Regis* (Oxford, 1961), 30. From an analysis of scribal hands, Bishop has identified two royal scribes active at Henry's accession in 1100; their number had risen at least to four by mid-reign and seems to have remained at about that level until Henry's death.

¹³ For Robert, see *Regesta Regum Anglo-Normannorum*, 2: x; and Johnson, *Dialogus de Scaccario*, 129. Of Robert's attestations 36 percent are from England, 44 percent from Normandy. His contemporary, the chancellor Geoffrey Rufus, attested 86 percent from England, 14 percent from Normandy. On the probable vacancy of the chancellorship after 1133, see Charles Johnson, "The Last Chancellor of Henry I," *EHR*, 67 (1952): 392.

¹⁴ David C. Douglas, *William the Conqueror* (London, 1964), 185–86, 207, *passim*; and Southern, *Medieval*

These administrative beginnings, aborted by Rufus's death in August 1100, foreshadowed the development of more stable and elaborate viceregal arrangements in the years following Henry's conquest of Normandy in 1106. From about 1107 onward the Norman viceregal court was headed by John, bishop of Lisieux, whose numerous attestations of royal charters disclose that he was almost constantly at Henry's side in Normandy but seldom crossed with the king to England. His name occurs at the heads of lists of Norman justices, usually followed by that of the seneschal or steward, Robert de la Haye, whose overwhelmingly Norman attestations suggest that his administrative responsibilities were limited primarily to the duchy.¹⁵ He was evidently the chief layman in the Norman viceregal court and its second in command. Others active in the Norman viceregency include the household officials William of Glastonbury and Robert of Courcy, whose names occur chiefly in the Norman records of the reign.¹⁶

Charles Homer Haskins rigorously examined the personnel of Henry's Norman courts more than sixty years ago, and little has since been added to his fundamental work.¹⁷ But it is now possible, through comprehensive attestation analyses, to sift out the predominantly Norman figures from the cross-Channel *curiales* whose names sometimes appear in Norman documents as judges and administrative officials. A panel of judges hearing a Norman plea of A.D. 1111 included Geoffrey, archbishop of Rouen (who attested chiefly in Normandy), and the Norman "viceroys"—John, bishop of Lisieux—along with five lay magnates, at least four of whom held lands and attested substantially on both sides of the Channel. Similar analyses of the other documents that Haskins assembled disclose a Norman viceregal core group that sometimes functioned on its own, sometimes expanded into a larger court of bishops and magnates, and usually merged into the king's traveling entourage when Henry was in Normandy. Even when on its own, the viceregal court was itinerant, meeting sometimes at Rouen, sometimes at Caen, and perhaps, with less frequency, elsewhere in the duchy. Its members spent most of their time in Normandy but on rare occasions one or another turned up in the royal

Humanism, 189. Bishop Walchelin died in January 1098, and Flambard was advanced to the bishopric of Durham in June 1099, before becoming sole regent. The sources are silent on Rufus's viceregal administration in Normandy.

¹⁵ On Henry's Norman viceregency, see Charles Homer Haskins, *Norman Institutions* (Cambridge, Mass., 1918), 88–104; and Lucien Valin, *Le Duc de Normandie et sa cour (912–1203)* (Paris, 1910), 108–09. Of John's attestations, 88 percent are from Normandy, and the percentage would doubtless have been higher still had Henry's Norman charters survived in numbers comparable to those of his English charters. Of Robert's attestations, 96 percent pertain to royal acts emanating from Normandy. He was active in the Norman administration from 1118 or earlier to the end of the reign; see *Regesta Regum Anglo-Normannorum*, 2: nos. 1183–1901 *passim*; and Table 4, below. Lord of La-Haye-du-Puits in western Normandy, he (or possibly a kinsman of the same name) held considerable lands in Lincolnshire worth £78 in 1086 and pardoned £8 of danegeld in 1130. For a brief biographical sketch, see John Le Patourel, *Normandy and England, 1066–1144* (Reading, 1971), 34–35.

¹⁶ Haskins, *Norman Institutions*, 88–89, 120, 307; and *Regesta Regum Anglo-Normannorum*, 2: nos. 1184, 1352, 1422, 1579, 1593. Also see *ibid.*, 1584.

¹⁷ See, however, Jean Yver, "Le développement du pouvoir ducal en Normandie de l'avènement de Guillaume le Conquérant à la mort d'Henri I, 1035–1135," *Atti del Convegno Internazionale di Studi Ruggeriani* (Palermo, 1955), 183–204; and Le Patourel, *Norman Empire*, 121–354 *passim*.

entourage in England, leaving their Norman viceregal duties to subordinates or other household officers.¹⁸

In England Queen Matilda usually served as regent during her husband's absences, although Archbishop Anselm may have occupied the position on one or more occasions between his return from exile in 1106 and his death in 1109. After Matilda's death in 1118 William Adelin, the royal son and heir, directed the English regency for a year.¹⁹ But these altogether traditional arrangements were supplemented by a clearly identifiable body of viceregal administrators: the witness lists of charters issued by Matilda and William as regents disclose that in England as in Normandy a viceregal court of relatively stable and expert membership was evolving. At its head was Henry's great administrator, Roger, bishop of Salisbury, who attested no less than nine of Matilda's and William Adelin's surviving acts and whose name, when it occurs with others, almost always heads the list. Other figures in the English viceregal group included Robert Bloet, bishop of Lincoln; Richard of Belmeis, bishop of London; Ranulf the Chancellor; the stewards Adam of Port and William of Courcy; the constables Walter of Gloucester and Nigel of Oilli; and the royal justice Ralph Basset.²⁰ The stewards and constables on this list are among the household officials who attested primarily from England. Indeed, the attestations of almost all of those in the English viceregal core group were very largely limited to royal acts issued in England. Ranulf the Chancellor was the only official who crossed the Channel at various times in the king's service, but even he attested many more *acta* in England than in Normandy. Clearly, then, the chancellor was in part a cross-Channel *curialis* and in part a participant in the English viceregency.

One extraordinarily illuminating document, as yet unpublished in full, provides a glimpse of the English viceregal court hearing pleas at Brampton, Huntingdonshire, in 1116, apparently in the queen's absence. The "judges" at this Brampton court included several of the men in Matilda's and William

¹⁸ The magnate-judges were Robert, count of Meulan and earl of Leicester; William of Warenne, earl of Surrey; Gilbert of Laigle, lord of Pevensey; and William the Chamberlain of Tancarville. A fifth judge, William of Ferrars, was doubtless a Norman figure but he did not attest sufficiently to be placed with certainty. Haskins, *Norman Institutions*, 91–92. John of Lisieux traveled to England, for example, in late 1126, presumably to join the deliberations on the royal succession that culminated in the Empress Maud's designation at Henry's 1126–27 Christmas court; *Regesta Regum Anglo-Normannorum*, 2: no. 1466. John of Lisieux and Robert de la Haye were both apparently with the king at York in late 1122; *ibid.*, no. 1338. Both John and Robert were absent because of illness from a Norman court of ca. 1129 where Robert, chaplain to the bishop of Lisieux, and the steward Robert of Courcy seem to have acted in their places; *ibid.*, no. 1584.

¹⁹ On Matilda, see *Regesta Regum Anglo-Normannorum*, 2: nos. 971, 1000–01, 1190, 1198; and R. R. Darlington, ed., *The Cartulary of Worcester Cathedral Priory (Register I)*, Pipe Roll Society (London, 1968), nos. 40, 262. On Anselm, see Eadmer, *Historia Novorum in Anglia*, 197; and F. S. Schmitt, ed., *S. Anselmi Opera Omnia* (Stuttgart, 1958), epistle 407. In early 1109 Henry conferred with Anselm on vital matters of royal diplomacy; *S. Anselmi Opera Omnia*, epistle 461. The queen probably visited Normandy in 1107, but she was with Anselm in England in 1108 or early 1109 when she issued the charter, on Anselm's advice, that established the Augustinian house of Holy Trinity, Aldgate; *Regesta Regum Anglo-Normannorum*, 2: nos. 808–09, 906. On William Adelin, see *ibid.*, nos. 1189, 1191–92, 1201–02.

²⁰ For Roger, see *Regesta Regum Anglo-Normannorum*, 2: nos. 906, 909, 1090, 1189–90, 1192, 1201; *Cartulary of Worcester Cathedral*, nos. 40, 262. Roger was the first or sole witness to all of these acts except number 906, for which he was the second of three episcopal attestors. For the English viceregal group, also see *Regesta Regum Anglo-Normannorum*, 2: nos. 971, 1001, 1129, 1180, 1191, 1198.

Adelin's regency administrations: Roger, bishop of Salisbury; Robert, bishop of Lincoln; Walter of Gloucester; and Ralph Basset.²¹ The witness lists of regents' charters, corroborated by the Brampton evidence, thus disclose a select and stable body of England-based viceregal administrators led by Roger, bishop of Salisbury, with functions and membership that closely parallel those of the Norman viceregal body headed by John, bishop of Lisieux.

In England, however, the queen or prince headed the viceregency. But the English viceregal court, and Roger of Salisbury in particular, exercised strong influence on Matilda and William Adelin. Herbert, bishop of Norwich, in begging a favor of Roger during one of Henry's absences, was confident that "you will not find our lady the queen difficult for . . . she takes advantage of your advice in all matters."²² After Matilda's death in 1118 and William Adelin's in 1120, Roger emerged from the shadows as the officially recognized head of the English viceregency. When Henry departed England in 1123, he appointed Roger to run the kingdom in his absence, and throughout the remainder of the reign Roger was responsible for "the doing of justice in England" not only during Henry's Norman tours but even when the king was in his kingdom. Despite Henry's marriage to the young Adeliza of Louvain in 1121, Roger—not the new queen—presided thenceforth at the English viceregal court.²³ Although Henry always felt free to issue writs concerning England when he was in Normandy (and vice versa), Bishop Roger himself sometimes issued viceregal writs explicitly on the king's instructions.²⁴

Neither Roger of Salisbury in England nor John of Lisieux in Normandy appear to have borne an official administrative title. Scholars in the past were inclined to call them "chief justiciars," but their administrative roles were too novel to have yet acquired formal names. Their episcopal titles sufficed.²⁵ At

²¹ The two remaining *iudicii* at Brampton were sheriffs of the region; Doris M. Stenton, *English Justice between the Conquest and Magna Carta* (Philadelphia, 1964), 62 n. 46. For a case involving the earl of Buckingham's obligations to Abingdon for a manor which he held of the abbey that Roger of Salisbury, Robert bishop of Lincoln, "et multis regis baronibus" judged, see Joseph Stevenson, ed., *Chronicon Monasterii de Abingdon*, Rolls Series, no. 1, vol. 2 (London, 1858): 133–34.

²² As quoted in Richardson and Sayles, *The Governance of Mediaeval England*, 151.

²³ *Anglo-Saxon Chronicle*, A.D. 1123; and Malmesbury, *Gesta Regum Anglorum*, 484. Roger's competition was further reduced by the deaths of Robert Bloet and Ranulf the Chancellor in 1123 and the paralysis that struck Richard, bishop of London, that same year. Henry probably made his decision out of dynastic rather than administrative considerations. Hoping that Queen Adeliza would bear him a son, he kept her at his side when he crossed to Normandy. Adeliza's attestations were balanced between duchy and kingdom, whereas Queen Matilda's were chiefly in England.

²⁴ *Regesta Regum Anglo-Normannorum*, 2: nos. 1472, 1488, 1614, 1814, 1977, 1989. Also see Francis J. West, *The Justiciarship in England* (Cambridge, 1966), 18–19; and Richardson and Sayles, *The Governance of Mediaeval England*, 163. None of Roger's viceregal charters are attested; four conclude with the phrase "per breve regis."

²⁵ For the term "chief justiciar," see, for example, Richardson and Sayles, *The Governance of Mediaeval England*, 159, *passim*; and Haskins, *Norman Institutions*, 87–99. *Capitalis justiciarius* does occur in Anglo-Norman sources but not with its later meaning. Orderic Vitalis used it to describe not Roger of Salisbury but Richard Basset and probably meant simply that Richard's judicial activities were wide-ranging; *Historiae Ecclesiasticae Libri Tredecim*, ed. Auguste le Prévost, 5 vols. (Paris, 1838–55), 5: 68. In 1141 the Empress Maud named Geoffrey de Mandeville her *capitalis justicia* in Essex. The term also occurs in a Norman charter of Henry I's but not explicitly in connection with John of Lisieux; Haskins, *Norman*

one point Roger titled himself “bishop of Salisbury and procurator of the kingdom of England under King Henry,” but no such title was consistently used. Contemporaries sometimes referred to him as “second only to the king” (*secundus a rege*)—surely not a formal office but an indication of his political importance.²⁶ For Roger’s authority in England after 1123 was remarkably comprehensive. As William of Malmesbury put it, Roger “pleaded the cases, controlled expenditures himself, personally supervised the treasure, both when the king was in England and also, without colleague or witness, when . . . the king was staying in Normandy.”²⁷ Roger’s control of the kingdom’s judicial and fiscal machinery strongly anticipated the power and responsibility exercised by the chief justiciars of early Angevin times. It is not too much to say that the varied responsibilities of both Roger of Salisbury in England and John of Lisieux in Normandy mark the genesis of the chief justiciarship in fact even if not in name.²⁸

ROGER’S ADMINISTRATION OF THE KINGDOM’S REVENUES was likewise exercised without formal title. The exchequer, perhaps the best-known cogwheel in Henry’s new administrative machine, emerges from the mist around 1110 with Roger of Salisbury clearly in control.²⁹ Despite extensive investigations, its origins remain obscure. The *Dialogus de Scaccario* from the end of the century implies that Roger did not invent the exchequer but did much to improve and modernize it.³⁰ Doubtless it evolved out of the ancient practice of sheriffs bringing their revenues annually to the central English treasury, located at Winchester since Anglo-Saxon times. Chroniclers alluded to a general administrative and legal reorganization undertaken by Henry on both sides of the Channel in the years immediately following his conquest of Normandy in 1106. This activity, combined with raising a huge aid for his daughter’s betrothal in 1110, may have stimulated the accounting reforms that produced what we know as the English exchequer—abacus accounting procedure,

Institutions, 93–94. Also see, in general, William T. Reedy, “Were Ralph and Richard Basset Really Chief Justiciars of England in the Reign of Henry I?” *The Twelfth Century, Acta*, 2 (1975): 74–103.

²⁶ “Justitiarius fuit totius Angliae, et secundus a rege”; Henry of Huntingdon, *Historia Anglorum*, ed. Thomas Arnold, Rolls Series, no. 74 (London, 1879), 245. Several of Roger’s contemporaries are likewise called justiciars “totius Angliae”; Richardson and Sayles, *The Governance of Mediaeval England*, 174–75. And Henry of Huntingdon himself describes Roger’s nephew, Alexander, bishop of Lincoln, as “princeps a rege secundus”; *Historia Anglorum*, 280. On Roger’s titles, see Edward J. Kealey, *Roger of Salisbury, Viceroy of England* (Berkeley and Los Angeles, 1972), 70–71, 241–44. On the various unofficial terms used to describe Ranulf Flambard’s duties under William II (*exactor*, *placitator*, etc.), see Southern, *Medieval Humanism*, 184–85, 194.

²⁷ William of Malmesbury, *Historia Novella*, ed. K. R. Potter (London, 1955), 37–38. Note that Roger’s viceregal charters always lack witnesses and that several of his earlier colleagues were dead or inactive by 1124; see notes 23–24, above.

²⁸ West, *Justiciarship*, 15–23. My position on Roger’s “chief justiciarship” is rather less skeptical than that of West but distinctly more so than that of Richardson and Sayles; see their *The Governance of Mediaeval England*, 173–90.

²⁹ Henry I to *baronibus de scaccario*, attested by Roger of Salisbury and Ranulf the Chancellor; *Regesta Regum Anglo-Normannorum*, 2: no. 963. This, the earliest known reference to the exchequer by name, deals with the aid of 1110.

³⁰ *Dialogus de Scaccario*, 42.

TABLE 2
Exchequer and Viceregency Officials, 1111–21

<i>Exchequer Court of 1111</i>	<i>Exchequer Judges of 1119</i>	<i>Viceregency Figures</i>
Queen Matilda* [E] Roger, bp. of Salisbury* [E] Robt., bp. of Lincoln* [E] Richard, bp. of London* [E] William of Courcy [E] Adam of Port [E] Thurstan the Chaplain [E] Walter of Gloucester* [E] Herbert the Chamberlain [E] (?) William of Oilli ^a Geoffrey fitz Herbert William of Anesy Ralph Basset* [E] Geoffrey of Mandeville [XN] Geoffrey Ridel [E] Walter, adcn. of Oxford	Roger, bp. of Salisbury* [E] Robt., bp. of Lincoln* [E] Ralph Basset* [E] Ranulf the Chancellor* [XE]	Queen Matilda* [E] Roger, bp. of Salisbury* [E] Robt., bp. of Lincoln* [E] Richard, bp. of London* [E] William of Courcy [E] Adam of Port [E] Walter of Gloucester* [E] Nigel of Oilli* [E] Ralph Basset* [E] Ranulf the Chancellor* [XE]

NOTE: An asterisk (*) designates a *curialis*; for a definition of a *curialis*, see page 887, below; and, for Henry's *curiales*, see Table 4, below. For an explanation of the symbols E, XE, and XN, see page 872 n. (Table 1), above. The absence of such a symbol indicates that the name does not occur elsewhere in Henry I's *Regesta*.

^a William of Oilli either was a little-known kinsman of Nigel of Oilli or appears on the list as a scribal error for Nigel himself.

court of audit, and pipe rolls recording receipts from the sheriffs and their shires.³¹

From about 1110 onward we encounter mounting evidence of the sophisticated accounting process that appears in some detail in the English pipe roll of 1130 (the one surviving example of the series of fiscal records that were produced annually throughout most of Henry's reign).³² And, although all of the Norman exchequer rolls from this period have perished, it is certain that an exchequer was functioning concurrently in the duchy. Up to a point, there is general agreement on how the exchequer process worked. Twice each year, at Easter and at Michaelmas, separate groups of royal administrators went to the Winchester treasury and to the Norman treasury to audit the accounts of

³¹ Orderic Vitalis, *Historiae Ecclesiasticae Libri Tredecim*, 4: 233–34, 269; Florence of Worcester, *Chronicon ex Chronicis*, ed. Benjamin Thorpe, 2 (London, 1849): 57; and Eadmer, *Historia Novorum in Anglia*, 192–93. And see R. L. Poole, *The Exchequer in the Twelfth Century* (London, 1912).

³² Joseph Hunter, ed., *The Pipe Roll of 31 Henry I, Michaelmas 1130* (hereafter *P.R. 31 Henry I*) (rev. ed.; London, 1929). Annual pipe rolls survive from A.D. 1156 onward and can be used (with caution) to illuminate the roll of 1130. Further illumination—along with some phantom images—is provided by Richard fitz Nigel's *Dialogus de Scaccario* from late in Henry II's reign.

the sheriffs and vicomtes over the famous checkered boards. Contemporary records referred to the members of these courts of audit as “barons of the exchequer,” and the title has given rise to some confusion because it suggests a body of full-time exchequer officials. Indeed, Lady Doris Stenton has referred to Henry I’s English exchequer as a “permanent financial bureau,” and Francis J. West has viewed it as “the central organ of government.”³³ Certainly, it became both in subsequent generations, but at its inception under Henry I the exchequer was merely a semiannual auditing procedure, nothing more or less than a highly effective device for increasing curial supervision over the sheriffs and vicomtes and a means of applying the latest systematic procedures to the collection of royal revenues and the dispensing of royal patronage. The exchequer was not yet a department, not yet an institution, but simply an occasion. Further, it was in essence the viceregal court meeting under special circumstances.

That Roger of Salisbury supervised both the viceregal court and the exchequer has long been understood. That other officials also served simultaneously as viceregal justices and barons of the exchequer has not. But the combined evidence of several of Henry’s charters makes it clear that the chancellor was, at least at times, a baron of the exchequer. And the panel of *justitiae regis* who in 1119 heard a plea for the exemption of Abingdon Abbey from danegeld almost certainly represents some of the exchequer barons sitting at the Michaelmas session at Winchester.³⁴ Without exception the justices in question—Roger of Salisbury; Robert, bishop of Lincoln; Ranulf the Chancellor; and Ralph Basset—are viceregency figures. An earlier passage from the Abingdon Chronicle preserves a full list of the officials who sat at the Winchester treasury during the Michaelmas exchequer session of A.D. 1111. The exchequer was so new at this point that the justices were not yet termed *barones de scaccario*, and the session itself was described as being *in thesauro* rather than *ad scaccarium*. But there can be no doubt that the account in question is the earliest recorded description of the exchequer court in action. The official regent of England, Queen Matilda, not Roger of Salisbury, presided over the court. But Roger’s name appears at the head of the list of judges, and these judges correspond remarkably to the men who assisted the queen and, later, her son in the English viceregency.³⁵ For the sake of clarity, Table 2 lists the members of the exchequer court of 1111 alongside the names of the exchequer justices of 1119 and those of the men identified with Matilda’s or with William Adelin’s regency.

Since the order in which the names appear in the Abingdon account of 1111 (and in Table 2) is doubtless indicative of the order of these officials’ impor-

³³ For *barones de scaccario*, see *Regesta Regum Anglo-Normannorum*, 2: nos. 963, 1538, 1584 (Normandy), 1741, 1879. And see Stenton, *English Justice*, 59; and West, *Justiciarship*, 19–20.

³⁴ On the chancellor, see *Regesta Regum Anglo-Normannorum*, 2: nos. 963, 1211, 1741. Also see *ibid.*, no. 1514. For the case in 1119, see *ibid.*, no. 1211. The editors propose the date of ca. September 29.

³⁵ *Ibid.*, no. 1000; and *Chronicon Monasterii de Abingdon*, 116.

tance, the common membership of viceregency and exchequer becomes all the more evident. Below the queen, six of the seven exchequer officials of 1111 were viceregency figures, and the one who was not, Thurstan the Chaplain, was a royal chaplain and a canon of St. Paul's under Bishop Richard of London. Of the remaining eight, Herbert the Chamberlain was present as custodian of the Winchester treasury, and Geoffrey Ridel as one of England's most active royal justices. As a "justiciar of all England," Geoffrey probably participated in regency courts even though his name does not occur in the handful of surviving viceregal records.³⁶ In short, a single administrative-judicial body traveled through England, hearing pleas in the king's name when he was in Normandy and sitting at the Winchester treasury at Easter and Michaelmas for the exchequer sessions. At such times its membership may have expanded to include a treasury chamberlain and perhaps some lesser figures, just as it expanded at the Brampton pleadings in 1116 to include two local sheriffs. Like all courts of its day, the members of the regency-exchequer *curia* shifted, but its core was remarkably stable. And its key figures, despite their English viceregal responsibilities, were frequent attestors of the king's charters. When Henry was in Normandy these officials served him from afar; when he was in England they were at his side.

The single surviving record of Henry I's Norman exchequer discloses a similar identity in membership between the viceregal and exchequer courts in Normandy. Among the barons of the Norman exchequer were the familiar figures of the Norman viceregency: John, bishop of Lisieux; the stewards Robert de la Haye and Robert of Courcy; and the chamberlain William of Glastonbury. One of the nonviceregency figures at the court was the Norman treasury official Robert of Évreux, whose presence paralleled that of Herbert, the treasury chamberlain, at Winchester in 1111.³⁷

As occasions rather than institutions, the exchequers had no permanent staffs. The officials of the English and Norman vicereencies, who for a time became "barons of the exchequer," met at their respective treasuries and stored their annual accounts there. Of the two treasuries, the one at Winchester was much the more important, for there Henry kept the bulk of his wealth unless he needed it in Normandy.³⁸ The Winchester treasury can be traced back to Canute's reign, but little is known of its administration until Henry I's accession. By then it was under the authority of two royal chamberlains—Herbert the Chamberlain and Robert Mauduit—both of whom were local

³⁶ Donald Nicholl, *Thurstan, Archbishop of York* (York, 1964), 7–10; and Reedy, "Were the Bassets Chief Justiciars?" 80–81. Of the remaining exchequer officials of 1111 who cannot be identified as viceregal administrators, Geoffrey of Mandeville (lord of Marshwood, Dorset) had recently been sheriff of Devon and Cornwall and possibly still was, Archdeacon Walter of Oxford appears seldom in official records and only in England, and the remaining men are altogether obscure. William of Oilli appears in no other contemporary record.

³⁷ For the one extant Norman exchequer record, see *Regesta Regum Anglo-Normannorum*, 2: no. 1584. Also see *ibid.*, nos. 1184, 1352, 1422, 1579, 1593. On Robert of Évreux, see Haskins, *Norman Institutions*, 108–10.

³⁸ For much of what follows, see C. Warren Hollister, "The Origins of the English Treasury," *EHR*, 93 (1978): 262–75.

Hampshire men only infrequently at court. But by the 1120s the two treasury chamberlainships had passed to the *curiales* Geoffrey of Clinton and William of Pont de l'Arche. Geoffrey had risen from obscurity to become a major landholder, sheriff, itinerant justice, and royal *familiaris*. William of Pont de l'Arche served off and on as sheriff in several shires and, in addition to his Winchester office, was chamberlain of the *camera curiae*, the subtreasury that traveled with the king's court.

The real master of the Winchester treasury was, of course, Roger of Salisbury himself, functioning without official title. In the mid-1120s, however, the new household office of court treasurer was created, and its authority extended beyond Winchester to all the treasuries of the Anglo-Norman state. Its first occupant was Roger of Salisbury's nephew Nigel, a frequent attestor of royal charters and the only member of his newly illustrious family to attest substantially from both sides of the Channel. His advancement to the new office represents the final step in the establishment of clearly defined curial control over the treasuries of both duchy and kingdom.

The English and Norman treasuries were depositories of records as well as wealth. Although memoranda of various kinds, now lost, doubtless accompanied the itinerant chancery, the age of major chancery records could not dawn until the chancery ceased traveling and settled down—as it did around 1200. But records had long been accumulating in the Winchester treasury. When Henry seized it in 1100 he must have found there not only Rufus's coins but the records necessary to keep them flowing in: Domesday Book with its comprehensive data on baronial holdings and the hidages and values of manors, pre-Domesday geld rolls, and records of the shire farms and of revenues owed by royal estates and towns. To these materials, Henry's own administration added the annual exchequer accounts—the English pipe rolls and Norman exchequer rolls. The king's two formal treaties with the count of Flanders, which survive in their original chancery hand, were evidently deposited at Winchester, along with early twelfth-century regional surveys that updated the Domesday information for Leicestershire, Northamptonshire, Worcestershire, and Lindsey. There is even scattered evidence that royal writs were sometimes copied and stored at Winchester.³⁹ Thus, despite the itinerant character of the king's *curia* and viceregency courts, the royal administration was beginning to store in fixed depositories the records necessary to account for what the crown was collecting and what it was granting away.

³⁹ In general, see Sally P. J. Harvey, "Domesday Book and Anglo-Norman Governance," *Transactions of the Royal Historical Society*, 5th ser., 25 (1975): 173–93. On the Norman exchequer rolls, see Thomas Stapleton, *Magni Rotuli Scaccarii Normanniae sub Regibus Angliae*, 2 vols. (London, 1840–44). For an apparent early reference (ca. 1114) to the preservation of pipe rolls, see Poole, *The Exchequer in the Twelfth Century*, 37–39; and *Regesta Regum Anglo-Normannorum*, 2: no. 1053. Richard fitz Nigel alluded to information that was available "in veteribus annalibus rotulis" of Henry I; *Dialogus de Scaccario*, 42. In ca. 1127 Henry stated that Roger of Salisbury had recognized by charter from the royal treasury ("per cartam de thesauro meo") that Plympton Priory (founded in 1121) was to be free of gelds and other levies; see *Regesta Regum Anglo-Normannorum*, 2: no. 1515. For similar evidence, see *ibid.*, no. 1488; for other types of royal charters that must surely have been on record at Winchester, see *ibid.*, nos. 1581, 1687, 1691.

THE RIGOROUS EXCHEQUER AUDITS of sheriffs' and vicomtes' accounts did much to solve the problem of controlling local officials, a problem that has beset governments in all ages. And the exchequer was not the only instrument that Henry directed to this end. By the later years of his reign the royal court exercised formidable authority over the shires and hundreds of England through a comprehensive system not only of itinerant curial justices but also of itinerant curial sheriffs. In Normandy the evidence is much thinner because all of the exchequer rolls and most of the royal charters have disappeared. But the surviving records reveal that royal justices were actively at work in the duchy and that the vicomtés of Avranches, Bayeux, Falaise, Argentan, and Exmes were governed at times by royal *familiares*.⁴⁰

How and to what extent Henry centralized English justice are matters of considerable debate. It is generally agreed that the responsibility for pleas of the crown was initially transferred from sheriffs to shire justiciars under William I or William II and that shire justiciarships became an established institution under Henry I. The institution is thought to have grown steadily throughout Henry I's reign and on into the 1150s and 1160s, dissolving only when Henry II's system of judicial eyres made shire justiciarships redundant.⁴¹ Meanwhile, so it is urged, Henry I supplemented the work of the local or shire justiciars by sending itinerant justices out from his court to hear pleas of the crown in various groups of shires. Nevertheless, as William T. Reedy has argued in his careful study of Henry I's itinerant justices, their activities were limited in scope and therefore quite unlike Henry II's "general eyres"; the king continued to exercise his jurisdiction "primarily through his local justiciarate," the history of which is marked, unfortunately, by "little concrete evidence."⁴²

Such a reconstruction of Henry I's judicial system is unsatisfactory in several respects. One is struck, to begin with, by the great proliferation of royal justices of all kinds under Henry I—justices of groups of shires, individual shires, hundreds, boroughs, and royal demesne lands.⁴³ To differentiate among these various sorts of justices can be treacherous, and a certain

⁴⁰ The *familiaris* Richard, earl of Chester, was vicomte of the Avranchin; the *familiaris* Ranulf le Meschin was vicomte of the Bessin and, after Richard of Chester's death, of the Avranchin as well; John, bishop of Lisieux, served for a time as vicomte of Falaise, Argentan, and Exmes. On the activities of royal justices in Normandy, see Haskins, *Norman Institutions*, 100; and Orderic Vitalis, *Historiae Ecclesiasticae Libri Tredecim*, 4: 439–40, 453.

⁴¹ Henry A. Cronne, "The Local Justiciar in England under the Norman Kings," *Birmingham Historical Journal*, 6 (1957): 18–38; Charles Johnson and Henry A. Cronne, *Regesta Regum Anglo-Normannorum*, 2: xvii; and Stenton, *English Justice*, 65–69.

⁴² Reedy, "The Origins of the General Eyre in the Reign of Henry I," *Speculum*, 41 (1966): 688–724. On the justices in eyre, also see Doris M. Stenton, ed., *Pleas before the King or His Justices, 1198–1212* (hereafter *Pleas, 1198–1212*), Selden Society, no. 85, vol. 3 (London, 1967): xlvii–l. On sheriffs, see W. A. Morris, *The Medieval English Sheriff to 1300* (Manchester, 1927), 41–109; and C. H. Walker, "The Sheriffs and the Pipe Roll of 31 Henry I," *EHR*, 37 (1922): 67–79.

⁴³ L. J. Downer, ed., *Leges Henrici Primi* (Oxford, 1972), 98, 132, 183, 195, 212; *P.R.* 31 Henry I, 91; Stenton, *English Justice*, 57; Orderic Vitalis, *Historiae Ecclesiasticae Libri Tredecim*, 3: 125, 4: 439–40; Kenneth R. Potter and R. H. C. Davis, eds., *Gesta Stephani* (2d ed., Oxford, 1976), 24; Naomi D. Hurnard, "Local Justice under the Norman Kings," in H. W. C. Davis, *England under the Normans and Angevins* (13th ed., London, 1949), 522–24; and Cronne, "Local Justiciar," 22.

confusion persists in the secondary literature between shire justiciars and other kinds of local justices.⁴⁴ Contemporaries had not yet developed a technical vocabulary to distinguish shire justiciars from itinerant justices: Henry addressed a writ to Aubrey de Vere and Robert of Chesney as his “justices of Norfolk,” but they were also justices in Suffolk, and Aubrey was apparently a justice at about the same time in Middlesex and Nottinghamshire; he was described by his son as “justiciar of all England.”⁴⁵ None of this would have seemed contradictory to people at the time: a “justiciar of all England,” of whom there were several concurrently, did not necessarily hear pleas in every shire or even in many shires. He simply had the authority to do so. And a writ relating to judicial business in Norfolk would be addressed to him in his capacity as justice of Norfolk, as his activities in other shires were irrelevant to the business at hand.⁴⁶

This and other evidence points to the difficulty of separating itinerant justices from shire justices. The distinction, clearly and persistently drawn by modern scholars, was not at all clear to contemporaries. Not until the 1170s was there a technical term for “itinerant justice.” Modern scholars have consistently included William of Houghton and Henry of Port among Henry I’s justices in eyre, but the surviving pipe roll shows Henry of Port hearing pleas only in Kent and William of Houghton only in Suffolk. Conversely, Geoffrey de Mandeville, earl of Essex, who has been taken by scholars as a prime example of a “local justiciar,” was granted judicial authority over three shires.⁴⁷ Although King Stephen granted Geoffrey’s judicial office “in feodo et hereditate” whereas Henry I’s justices seem to have served at the royal pleasure, this distinction is not an altogether satisfactory reason to call Geoffrey’s justiciarship “local” and those of Henry of Port and William of Houghton “itinerant.” It has been assumed that itinerant justices were sent out from the *curia regis* whereas shire justices were planted in their shires. But

⁴⁴ Cronne has recognized the danger but not always sufficiently. Having assembled evidence on every sort of English royal judicial official functioning within the confines of a shire, he has concluded that the office of “shire justiciar” was firmly established at the time of Henry I’s death; “Local Justiciar,” 32, 37–38. And Cronne and R. H. C. Davis have maintained elsewhere, “It must be assumed that every county had its local justice as a matter of course”; *Regesta Regum Anglo-Normannorum*, 3: xxiii.

⁴⁵ *Regesta Regum Anglo-Normannorum*, 2: nos. 1714, 1772, 1988. And see Richardson and Sayles, *The Governance of Mediaeval England*, 174. Cronne and R. H. C. Davis have identified Aubrey as a “local justice” of Norfolk and Suffolk, but Stephen’s charters are quite ambiguous on this point; compare *Regesta Regum Anglo-Normannorum*, 3: xxv, and *ibid.*, nos. 82, 416.

⁴⁶ *Regesta Regum Anglo-Normannorum*, 2: no. 1608; and Barbara Dodwell, ed., *The Charters of Norwich Cathedral Priory, Part One*, Pipe Roll Society (London, 1974), no. 99. In this royal writ relating to Norfolk, Richard Basset and Aubrey de Vere were addressed as *justiciarii*; Cronne and Johnson have dated the charter “1129?” and Barbara Dodwell has suggested a date “probably not long before Michaelmas 1129,” on the grounds that Richard and Aubrey became joint sheriffs of Norfolk and Suffolk at Michaelmas 1129; see *P.R.* 31 *Henry I*, 90. I suggest that the charter should be dated somewhere between 1130 and 1133 and that Richard and Aubrey were sheriffs and justiciars concurrently. Norfolk was among the six shires in which Richard Basset heard pleas ca. 1129–30, and Aubrey probably began serving as an itinerant justice shortly thereafter.

⁴⁷ For *justice errantes*, see R. C. Van Caenegem, *The Birth of the English Common Law* (Cambridge, 1973), 21. For William of Houghton and Henry of Port, see Stenton, *English Justice*, 62; Reedy, “General Eyre,” 712; and *P.R.* 31 *Henry I*, 65, 96. For Geoffrey de Mandeville, see Cronne and R. H. C. Davis, *Regesta Regum Anglo-Normannorum*, 3: xxiii–xxv, nos. 274, 275; Cronne, “Local Justiciar,” 21–23; and Stenton, *English Justice*, 66. Geoffrey was granted both judicial and shrieval authority over Essex, Hertfordshire, and Middlesex.

the assumption cannot be sustained when Aubrey de Vere—a heavily attesting *curialis* and joint sheriff of eleven shires—is regarded as the shire justiciar of Norfolk while Robert of Arundel—who seldom attested—is regarded as an itinerant justice in the west.⁴⁸

These problems suggest the need to revise the existing picture of Henry I's judicial system. The belief that shire justiciars, as traditionally conceived, grew and flourished throughout the reign should be reassessed. There were always, of course, minor justices working in the shires and hundreds—men such as “Benjamin” in Norfolk and Robert Malarteis in Huntingdonshire, who impleaded suspected criminals and attended to the king's judicial affairs in various other ways. There were also justiciars of individual shires, but efforts to identify them specifically have resulted in lists of men who were active only in the earlier portion of the reign, and most of the references date from its initial years. Midway through the reign their activities began to blend into those of the “itinerant justices.” Ralph Basset, whose judicial activities left their mark in at least eleven shires in the roll of 1130, had earlier been on eyre in two others: Huntingdonshire in 1116 and Leicestershire in 1124.⁴⁹ We know of his activities in these years only through isolated chronicle references, but for the period roughly from 1125 to 1130 the pipe roll provides a relatively full picture.

It could be fuller still. Several shire accounts are missing while others are incomplete or mutilated.⁵⁰ The roll may, moreover, sometimes conceal the names of itinerant justices under rubrics like “old pleas,” “pleas for breaking the peace,” or “the old pleas of Holderness.”⁵¹ Nevertheless, the roll makes it clear that between about 1125 and 1130 royal justices were at work in all or nearly all of the shires of England.⁵² Not every justice was as active as Ralph Basset (eleven shires) or Richard Basset (six shires) or Geoffrey of Clinton (eighteen shires). The names of two justices, for example, occur only in single

⁴⁸ Stenton, *Pleas, 1198-1212*, xlix; and Reedy, “General Eyre,” 720-21.

⁴⁹ R. C. Van Caenegem, “Public Prosecution of Crime in Twelfth-Century England,” in C. N. L. Brooke et al., eds., *Church and Government in the Middle Ages: Essays Presented to C. R. Cheney* (Cambridge, 1976), 51-61; Richardson and Sayles, *The Governance of Mediaeval England*, 185-87; Cronne and Johnson, *Regesta Regum Anglo-Normannorum*, 2: xviii; and Cronne, “Local Justiciar,” 33. For one of many references to *judices* of the county and hundreds, see *P.R. 31 Henry I*, 97. It has been my good fortune to have access to a much fuller and more rigorous list of local justiciars; see Hoc-ming Cheung's unpublished paper, “Local Justiciars under Henry I” (University of California, Santa Barbara). For Ralph Basset, see *Anglo-Saxon Chronicle*, A.D. 1124; and Orderic Vitalis, *The Ecclesiastical History of Orderic Vitalis*, ed. and trans. Marjorie Chibnall, 4 vols. (vols. 2-5) (Oxford, 1969-75), 3: 351. Since the reference to Ralph at Huntingdon in 1116 occurs in Orderic's account of the case of Bricstan of Chatteris, Reedy has concluded that Ralph “did not hear ‘pleas’” but only one plea; “General Eyre,” 705. But Bricstan's indictment for usury and concealment of treasure was a matter of only routine interest to the *curia regis* and would surely not alone have drawn together the great court that Orderic described.

⁵⁰ The accounts for Somerset and several border shires are missing altogether; little is left of the Devon account, and an entire membrane has been lost from the Hampshire account.

⁵¹ *P.R. 31 Henry I*, 3 (“old pleas”), 104 (“old pleas of Richard Basset”), 26 (“old pleas of Holderness”), 25 (“pleas of Blyth”), 27 (“pleas of Geoffrey of Clinton and his fellows of Blyth”), 45-46 (pleas “pro pace fracta”), 74 (pleas of Geoffrey of Clinton “pro pace fracta”), etc.

⁵² Reedy has surely erred in saying that “six counties out of those reported were not visited at all”; “General Eyre,” 715 n. 164. The pipe roll cannot be used to prove that shires were not visited. Of Reedy's six, Hampshire has only a partial account and Rutland was not a shire in 1130. The accounts for all six include entries that could well refer to the pleas of itinerant justices, even though they are unnamed.

shires, though the scope of their activities may well have been larger.⁵³ But taken together the pipe roll and charter evidence reveal that shire justiciars had evolved by the 1120s into justices with larger responsibilities—men who would have been described by a later generation as “justices in eyre.”⁵⁴ The eyre system appears to have collapsed with Roger of Salisbury’s arrest and death in 1139, and it was replaced by shire justiciarships and earldoms in the time of civil war that followed. King Stephen granted justiciarships of shires or small groups of shires, sometimes on a hereditary basis, to notables such as the earls of Essex and Lincoln and the bishop of Lincoln as well as to lesser men.⁵⁵ Finally, a decade or two into Henry II’s reign, the shire justiciarship was swallowed up by the judicial eyres and vanished.⁵⁶ I contend that the shire justiciarship was already dissolving under Henry I—and for the same reason.

Henry’s policy toward his sheriffs followed a similar pattern. Shrievalties and justiciarships underwent parallel transformations as the king and his *curia* tightened their control. Generally speaking, Henry turned away from his father’s policy of appointing magnates as sheriffs and instead appointed less exalted, more pliable men. Sheriffs were shuffled constantly in and out of shires, and as his reign progressed Henry increasingly tended to appoint *curiales* to the office. The pipe roll of 1130 discloses an extraordinary experiment in centralization: two of Henry’s most trusted curial administrators, Richard Basset and Aubrey de Vere, were by then joint sheriffs of no less than eleven shires, while many of the remaining shires were in the hands of *curiales* such as the constable Miles of Gloucester and the treasury chamberlains Geoffrey of Clinton and William of Pont de l’Arche. All but the last of these men were active concurrently as itinerant justices and were sometimes to be found on judicial eyres in the very shires that they held as sheriffs (see Table 3).⁵⁷ Indeed, the joint sheriffs Basset and Vere were, to all intents and purposes, “sheriffs in eyre.” They did not farm their shires in the traditional manner but seem to have functioned instead as *custodes*, responsible to the king

⁵³ We know of William of Houghton’s activity in Suffolk through only a single entry, recording what appears to be a rather old debt: *P.R. 31 Henry I*, 96. But a charter of ca. 1127 shows him working as a royal justice in Bedfordshire: *Regesta Regum Anglo-Normannorum*, 2: no. 1505. Geoffrey of Clinton’s work in Sussex is likewise enshrined in a single, barely legible entry in the roll; *P.R. 31 Henry I*, 69.

⁵⁴ The single known exception to this trend was Henry I’s charter to the citizens of London (ca. 1133) that granted to them the privilege of electing a justice to supervise crown pleas and pleadings in London and Middlesex: *Regesta Regum Anglo-Normannorum*, 2: no. 1645. Although the authenticity of this charter has recently been questioned, I continue to regard it as a genuine act of Henry I.

⁵⁵ Cronne and R. H. C. Davis, *Regesta Regum Anglo-Normannorum*, 3: xxiv–xxv, nos. 276, 472, 490. In this last writ (A.D. 1154) Stephen conceded to Robert of Chesney, bishop of Lincoln, “justitiam meam . . . de Lincolnescira” as fully as it had been enjoyed by his predecessors Robert Bloet and Alexander. The pipe roll of 1130 shows that pleas were held in Lincolnshire by Ralph Basset, Geoffrey of Clinton, William of Albany Brito, and Richard Basset, but not by Bishop Alexander. Also see the reference to “the pleas of the bishop of Lincoln” in the Lincolnshire account for 1155; Joseph Hunter, ed., *The Great Roll of the Pipe for the Second, Third, and Fourth Years of the Reign of Henry II*, Record Commission (London, 1844), 26.

⁵⁶ Stenton, *English Justice*, 68. The eyres of Henry II’s later years were not, however, “general eyres” in the sense in which Reedy has understood the term.

⁵⁷ Morris, *The Medieval English Sheriff*, 75–104. Miles of Gloucester was both sheriff and itinerant justice in Gloucestershire and Staffordshire, Richard Basset was joint sheriff and justice in Hertfordshire, Leicestershire, Norfolk, and Suffolk, and Geoffrey of Clinton did the same double duty in Warwickshire.

TABLE 3
Sheriffs and Royal Justices

<i>Shires</i>	<i>Sheriffs (1129-30)</i>	<i>Royal Justices (c. 1124-30)</i>
Bedes.:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*
Berks.:	Wm. of Pont de l'Arche*	Geoff. of Clinton*; Ralph Basset*
Bucks.:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*; Ralph Basset*
Cambs.:	Richard Basset* & Aubrey de Vere*	? ? ? ^a
Cornw.:	Geoff. of Furnell	(?) Robt. Arundel
Cumb.:	Hildred of Carlisle	Walter Espec/Eustace fz. John*
Derby:	Osbert Silvan	Geoff. of Clinton*; Ralph Basset*
Devon:	Geoff. of Furnell	Robt. Arundel
Dorset:	Warin	(?) Robt. Arundel
Essex:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*
Glous.:	Miles of Gloucester*	Miles of Gloucester*/Payn fz. John*
Hants.:	Wm. of Pont de l'Arche*	? ? ? [membrane missing from P. R. 1130]
Heref.:	(?) Payn fz. John*	(?) Payn fz. John* [missing from P. R. 1130]
Herts.:	Richard Basset* & Aubrey de Vere*	Richard Basset*
Hunts.:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*
Kent:	Rualon of Avranches	Geoff. of Clinton*; Henry of Port
Leics.:	Richard Basset* & Aubrey de Vere*	Ralph Basset*; Richard Basset*
Lincs.:	Rayner of Bath	Geoff. of Clinton*; Richard Basset*; Ralph Basset*; Wm. of Aubigny <i>Brito</i> *
Mdsx.:	Four Londoners	Ralph Basset*
Norf.:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*; Richard Basset*; Ralph Basset*
Northants.:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*
Northumb.:	Odard of Bamborough	Walter Espec/Eustace fz. John*
Notts.:	Osbert Silvan	Geoff. of Clinton*; Ralph Basset*
Oxford:	Robt. of Chesney	? ? ?
Rutland:	Wm. of Aubigny <i>Brito</i> ^b	? ? ?
Salop.:	Payn fz. John*	(?) Payn fz. John* [missing from P. R. 1130]
Somers.:	Warin	? ? ? [missing from P. R. 1130]
Staffs.:	Miles of Gloucester*	Geoff. of Clinton*; Miles of Gloucester*; Payn fz. John*
Suffolk:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*; Wm. of Houghton; Richard Basset*; Ralph Basset*
Surrey:	Richard Basset* & Aubrey de Vere*	Geoff. of Clinton*; Ralph Basset*
Sussex:	Hugh of Warelville	Geoff. of Clinton*; Richard Basset*
Warwics.:	Geoff. of Clinton*	Geoff. of Clinton*
Wilts.:	Warin	Geoff. of Clinton*; Ralph Basset*; (?) Robt. Arundel
Worcs.:	(?) Walter of Beauchamp	? ? ? [missing from P. R. 1130]
Yorks.:	Bertram of Bulmer	Geoff. of Clinton*; Ralph Basset*; Walter Espec/Eustace fz. John*

NOTE: An asterisk (*) designates a *curialis*.

^a Richard Basset and Aubrey de Vere were serving as royal justices in Cambridgeshire sometime between 1133 and 1135; see F. O. Blake, ed., *Liber Eliensis* (London, 1962), 287-88.

^b Rutland was not regarded as a "shire" in 1130; William of Aubigny *Brito* was responsible for its farm but was not, strictly speaking, its "sheriff."

for the whole of the royal revenues.⁵⁸ This intense degree of curialization may have been relaxed slightly during the closing years of the reign, but the sheriffs remained to the end under the strictest royal control.

SUCH ARE THE CONTOURS of Henry I's administrative machine. At its center was the itinerant royal *curia* of household officials and *familiares*. Ranging outward were the English and Norman vicereencies with their semiannual exchequer sessions, and the fixed treasuries with their chamberlains, coins, and records. At the grassroots level were the sheriffs, vicomtes, and local justices whose responsibilities, at least in England,⁵⁹ passed more and more under the expert supervision of *curiales* exercising authority over large regions. Overall, the reign was marked by an ever-increasing concentration of authority in the hands of an elite group of *curiales* who gave the system its cohesion.

The growth of curial control can be examined with some precision by making a comprehensive survey of attestations of royal charters. Use of this methodology has helped establish that members of the English viceregency court were very seldom in Normandy and vice versa.⁶⁰ The same technique helps identify the royal *curiales*—that is, men whose attestations demonstrate frequent participation in the king's court. For the purposes of this analysis, I have arbitrarily limited the term *curialis* to the thirty-one men active in 1130 who attested surviving authentic charters at the rate of at least two per year, and to a group of twenty-four active in 1111 who attested one and one half or more charters per year.⁶¹ Table 4 ranks these *curiales* in descending order of attestations made per year. These criteria thus enable us to measure the intensity of curial control over Henry's administration in 1130, when the pipe roll casts its shaft of light.

The results are these: of the seven justices in eyre whose responsibilities are known with certainty to have extended beyond a single shire, six were *curiales* (see Table 3). As a consequence of the great centralization of shrievalties disclosed by the surviving pipe roll, *curiales* served as sheriffs in over half of the English shires.⁶² Of the fifteen justices of the 1111 exchequer court, the first eight (presumably listed in order of importance) were all *curiales* or household officials or both, and the four (exchequer?) justices of 1119 were all *curiales*. The only extant record relating to the Norman exchequer discloses the same

⁵⁸ J. H. Round, *Geoffrey de Mandeville* (London, 1892), 297–98; and *P.R.* 31 *Henry I*, 63.

⁵⁹ The disappearance of all Henry I's Norman exchequer rolls leaves the situation in Normandy unclear. A passage from the Troarn cartulary shows the "king's justice" William Tanetin first sitting at Caen with John of Lisieux and later settling the case on his own; Haskins, *Norman Institutions*, 98.

⁶⁰ See pages 871, 873, and Tables 1, 2, above.

⁶¹ I have not calculated raw totals but totals divided by the span of years across which a person is known to have attested: Roger Bigod, who attested 59 royal charters over seven years, must be regarded as a more active *curialis* than William of Warenne who attested 69 royal charters over thirty-three years. In my calculations of overall appearances at court I have not made allowances for the greater survival rate of English to Norman charters, and the court appearances of predominantly Norman attestors have therefore been systematically underestimated.

⁶² Eighteen or nineteen of the thirty-five reported shires were in the hands of *curiales*; see Table 3. The noncurial sheriffs of 1130 are minor or middling landholders and administrative functionaries.

TABLE 4
The Most Frequent Attestors of Royal Acts

Rank	Attestors Active in A.D. 1111					Attestors Active in A.D. 1130				
	Name	Total Attested	Year Range	Av./ Yr.		Name	Total Attested	Year Range	Av./ Yr.	
1.	Ranulf, Chancellor	168	XE	07-22	11.5	Geoffrey, Chancellor	115	XE	23-33	11.5
2.	Roger, bp. of Salisbury	247	E	00-35	7.1	Roger, bp. of Salisbury	247	E	00-35	7.1
3.	Robt., bp. of Lincoln	155	E	00-23	6.7	Robt. <i>de Sigillo</i>	95	X	21-35	6.8
4.	Robt., ct. of Meulan	113	XE	00-18	6.3	Henry, bp. of Winchester	23	E	29-33	5.8
5.	Nigel of Aubigny	148	XE	01-29	5.3	Robt., e. of Gloucester	79	X	21-35	5.6
6.	Eudo the Steward	68	XE	00-15	4.5	Humphrey of Bohun	27	X	30-35	5.4
7.	Geoffrey of Clinton	90	X	10-32	4.1	Miles of Gloucester	43	E	26-35	4.8
8.	Wm. of Tancarville	89	X	07-29	4.0	Geoffrey of Clinton	90	X	10-32	4.1
9.	Queen Edith-Matilda	65	E	00-18	3.6	Brian fitz Count	41	XN	25-35	4.1
10.	Wm. of Aubigny <i>Pincerna</i>	120	X	00-35	3.4	Robt. de Vere	49	X	21-35	3.5
11.	Geoffrey fitz Payn	74	XN	11-35	3.1	Nigel the Treasurer	22	X	26-35	3.4
12.	John, bp. of Lisieux	69	N	07-35	2.5	Wm. of Aubigny <i>Pincerna</i>	120	X	00-35	3.4
13.	Hamo the Steward	72	XE	00-29	2.5	Hugh Bigod	47	X	21-35	3.4
14.	Henry, e. of Warwick	39	E	00-18	2.2	Thurstan, apb. of York	62	X	14-33	3.2
15.	Ralph Basset	41	E	10-29	2.2	Geoffrey fitz Payn	74	XN	11-35	3.1
16.	Wm., bp. of Winchester	57	XE	00-27	2.1	Payn fitz John	60	XE	15-35	3.0
17.	Wm. of Warenne ^a	69	X	00-35	2.1	Richard Basset	27	E	26-35	3.0
18.	Richard, bp. of London	23	E	08-21	1.8	Wm., abp. of Canterbury	35	XE	23-35	2.9
19.	Walter of Gloucester	31	E	10-28	1.7	Wm. of Pont de l'Arche	35	X	21-33	2.9
20.	Geoffrey, abp. of Rouen	29	XN	11-28	1.7	Hugh, abp. of Rouen	17	N	29-35	2.8
21.	Gilbert of Laigle	29	XE	01-18	1.7	Alex., bp. of Lincoln	34	E	23-35	2.8
22.	Wm., bp. of Exeter	30	XE	07-25	1.7	Bernard, bp. of St. David	52	X	15-35	2.6
23.	Ranulf, bp. of Durham	43	XE	01-27	1.7	Aubrey de Vere	36	XE	21-35	2.6
24.	Nigel of Oilli	23	E	01-16	1.5	John, bp. of Lisieux	69	N	07-35	2.5
25.						Audoin, bp. of Évreux	39	N	19-35	2.4
26.						Eustace fitz John	26	E	21-33	2.2
27.						Wm. of Warenne ^a	69	X	00-35	2.1
28.						Wm. of Aubigny <i>Britto</i>	30	E	20-35	2.0
29.						Robt. de la Haye	38	N	16-35	2.0
30.						John fitz Gilbert	12	X	29-35	2.0
31.						Robt., bp. of Hereford	8	E	31-35	2.0
32.						Wm. fitz Odo	26	N	21-35	1.9
33.						John, bp. of Sées	18	N	25-35	1.8
34.						Gilbert, bp. of London	7	E	29-33	1.8
35.						Robt. of Courcy	37	XN	13-35	1.7
36.						Wm. Martel	8	X	28-33	1.6
37.						Waleran, ct. of Meulan ^b	14	X	19-35	1.6
38.						Robt., e. of Leicester	23	X	20-35	1.5

NOTE: Under total royal acts attested, E = 91-100 percent English attestations; XE = 81-90 percent English; X = 51-80 percent English; XN = 50-64 percent Norman; N = 65-100 percent Norman. (Some adjustment has been made for the greater survival expectancy of charters from England; pre-1106 charters have been excluded from the English versus Norman analysis.)

^a William of Warenne was exiled from England between 1101 and 1103, and these years have been omitted from the calculation of the average.

^b During the years 1122-28 Waleran, count of Meulan, was first a rebel, then a captive, and those years have been omitted from the calculation of the average.

concentration of authority in the hands of household officials and *curiales*.⁶³ The principal Norman regents—John, bishop of Lisieux, and the steward Robert de la Haye—were both *curiales*. So was Roger of Salisbury, along with the more active of his associates in the English viceregency.⁶⁴ The Winchester treasury chamberlains in 1130 were both *curiales*, as was Nigel the Treasurer, and Henry I had the Winchester treasury audited that year by two more *curiales*: his own well-educated bastard Robert, earl of Gloucester, and Brian fitz Count, royal constable and lord of Wallingford.⁶⁵ The treasury audit reveals that Henry's *curia* supervised not only England and Normandy but itself as well. The power of the *curiales* was not unlimited, and administrative lapses could bring swift reprisals. Aubrey de Vere was fined for permitting a prisoner to escape his custody; Robert, bishop of Lincoln, who had somehow fallen from royal favor toward the end of his career, lost his tax exemptions and lawsuits along with his influence; and Geoffrey of Clinton was put under arrest for a time.⁶⁶ Yet as a group these men basked in the king's favor and prospered enormously. They were the chief technicians and the chief beneficiaries of Henry's administrative machine. By 1130 they ran nearly everything.

What sort of people were they? Historians have previously stressed Henry's policy of creating new men—"raised from the dust," as Orderic Vitalis put it. But the reality is more complex. There is abundant evidence to document Henry's favor toward old Conquest families: Beaumont, d'Avranches, Warrenne, Clare, Boulogne, and others. Conversely, the lands of certain other Conquest families were forfeited to the king on grounds of treason: Montgomery, Mortain, Grandmesnil, Abitôt, Lacy, Montfort, Malet. These forfeitures, usually ascribed to Henry's "ruthlessness," can more usefully be viewed as the final phase of a prolonged process of shuffling and reshuffling that constituted the post-Conquest land settlement—a process that involved repeated confiscations and new grants across the years between 1066 and about 1113.⁶⁷ Thereafter, the English holdings of baronial families became steadily more secure and entrenched. Henry's *curiales* included members of Conquest fami-

⁶³ Compare Tables 1 and 4, above.

⁶⁴ Robert of Lincoln, Richard of London, Ranulf the Chancellor, Walter of Gloucester, and Ralph Basset were all in the curial group; see pages 873, 879–80, above, and compare Tables 2 and 4, above.

⁶⁵ *P.R.* 31 *Henry I*, 130.

⁶⁶ Southern, *Medieval Humanism*, 217–19, 224–25; and *P.R.* 31 *Henry I*, 53.

⁶⁷ Orderic Vitalis, *The Ecclesiastical History*, 5: 296. Henry attempted to marry one of his bastard daughters to William of Warrenne, earl of Surrey, and later granted him the strategic fief of Saint-Saens in Normandy; C. Warren Hollister, "The Taming of a Turbulent Earl: Henry I and William of Warrenne," *Reflexions Historiques*, 3 (1976): 83–91. The king likewise offered his sister-in-law, Mary of Scotland, in marriage to William, count of Mortain and earl of Cornwall; on William's refusal Mary was wed to Eustace, count of Boulogne, another great landholder in England; *ibid.*, 85. Henry, earl of Warwick, and Robert, count of Meulan, as well as various members of the Clare family grew wealthier through Henry's favor; one bastard son of Hugh d'Avranches, earl of Chester, was raised to an abbacy and another, Othuer, was given extensive lands and a wealthy widow in marriage; Hollister, "Mandeville," 21–24. Great magnates had been dispossessed by Henry I's predecessors in 1073, 1082, 1088, and 1095. For a discussion of the gradual nature of the post-Conquest land distribution, see Le Patourel, *Norman Empire*, 40–45; and, for a demonstration of tenurial stabilization in the second half of Henry I's reign, see Gena de Aragon's unpublished analysis, "The Growth of Secure Inheritance in Norman England" (University of California, Santa Barbara).

lies—Warrenne, Bigod, and Beaumont—as well as great landholders more recently enriched—Robert of Gloucester and Brian fitz Count, “new men” to be sure, yet hardly “raised from the dust.” Besides the princely bastards Robert and Brian, the curial group included men of more obscure origins, several of whom Henry had elevated to magnate status by granting them lands and heiresses: Eustace and Payn fitz John, Miles of Gloucester, Geoffrey of Clinton, Richard Basset, and others. The group likewise included great prelates: the archbishops of Canterbury, Rouen, and York and the bishops of Lincoln, Winchester, St. David’s, Évreux, and, of course, Lisieux and Salisbury. Many of these curial prelates had themselves risen from the dust—most notably Roger of Salisbury and his nephew, Alexander of Lincoln. But, although the meteoric ascent of such men provoked some contemporary comment and grumbling, Henry’s policy was not simply to put down magnates and elevate new men in their places. The scattering of the post-Conquest estates presented him with the opportunity of cajoling and manipulating magnates with rewards and punishments similar to those applied to men of less exalted status.⁶⁸ Henry astutely arranged it that a great many of his wealthiest landholders were also royalists and *curiales*, and he accomplished this feat both by making magnates of his *curiales* and by making *curiales* of his magnates—luring them into his court and administration.

Accordingly, it is altogether misleading to view Henry’s *curiales* as a clique of smallholding royal administrators pitted against the great landholders. On the contrary, the *curia* included a good percentage of England’s wealthiest lay and ecclesiastical tenants-in-chief, some of whom had been reared from childhood in Henry’s court or elevated to prelacies from the staff of royal chaplains.⁶⁹ By 1130, at least half of the ten wealthiest English landholders were also the king’s *curiales*. This meld of wealth and service helps explain the *curiales*’ remarkable hold on Anglo-Norman administration and politics and the absence of serious opposition to Henry’s government in England. It also suggests a major qualification to the traditional notion of competition among honorial, ecclesiastical, and royal jurisdictions; for many of Henry’s wealthiest barons and prelates were also among his more active *curiales*. Drawn to the king’s side by the tug of their own ambitions, the *curiales* were, with rare exceptions, devoted royalists. They viewed the advancement of Henry’s interests and their own as two sides of the same coin. Long after Henry’s death Gilbert Foliot reminded the curial magnate Brian fitz Count of the “good and golden days” when King Henry “reared you from boyhood, educated you, knighted you, enriched you.”⁷⁰

⁶⁸ See, for example, Hollister, “Taming of a Turbulent Earl,” 83–91.

⁶⁹ On the royal chaplains’ near monopoly of major prelacies during much of the reign, see M. Brett, *The English Church under Henry I* (Oxford, 1975), 104–12.

⁷⁰ Gilbert Foliot, *The Letters and Charters of Gilbert Foliot*, ed. Adrian Morey and Christopher Brooke (Cambridge, 1967), 61; and Southern, *Medieval Humanism*, 220 (Southern’s translation).

HENRY I'S REIGN CONTRIBUTED MUCH to the development of English medieval institutions: it witnessed the origin of the exchequer and justices in eyre, and the concentration in Roger of Salisbury's hands of authority over exchequer, judicature, and viceregency—an authority that prefigured the later chief justiciarship. But subsequent kings, despite the machine's steady growth, had less success with it than Henry I. He was solvent, they were not—at least from the thirteenth century on. He kept the peace as few of them were able to do. And he achieved it not simply by developing the new administrative machinery but by placing it firmly under the control of some twenty or thirty *curiales*. In their hands the machine kept the treasury full for the defense of the Anglo-Norman state against domestic and foreign enemies. Just as important, it enabled Henry I to tighten his hold on his dominions and to enforce royal justice as never before.

PHILIP AUGUSTUS

IN 1165 KING LOUIS VII dubbed his newly born son, Philip, "Dieudonné" (given by God). After thirty years God had finally answered the king's prayers for a male heir. But the contemporary royal historian, Rigord of Saint-Denis, provided the sobriquet—"Augustus"—and the explanation that have survived to the present. Philip earned the title, Rigord declared, by "augmenting" the realm and its revenues as had the ancient Caesars.⁷¹ Philip inherited from his father a minuscule royal domain, popularly called the Ile-de-France, with centers at Orléans, Paris, and scattered points to the northeast of Paris. By marriage negotiations he gained the territories of Vermandois, Picardy, and Artois reaching northeast to the borders of Flanders. (The acquisition of Vermandois first prompted Rigord to invent the sobriquet.) By force of arms Philip expelled the English from northwestern France, driving them south of the Loire valley. Although the French kings could not maintain their hold on all of this land, the great duchy of Normandy was permanently annexed, which brought the royal domain to at least three—and perhaps four—times its former size. Philip confirmed these acquisitions by decisive victories on the battlefields of Roche-au-Moine and Bouvines in 1214.

In contrast to these dramatic territorial gains, the concomitant governmental adjustments to accommodate the new lands went virtually unperceived by contemporary chroniclers, including the royal historians. On Philip's death in 1223 Rigord's continuator, William the Breton, assessed the reign in traditional eulogistic language. Not only had the king marvelously broadened the rights and power of the French kingdom and filled the royal treasury, as Rigord had stated, but Philip had also pacified the great magnates and defended the churches.⁷² The great changes in administration found

⁷¹ Rigord, *Gesta Philippi Augusti*, in Henri-François Delaborde, ed., *Oeuvres de Rigord et de Guillaume le Breton*, 2 vols. (Paris, 1882–85), 1: 6–7.

⁷² William the Breton, *Gesta Philippi Augusti*, in Delaborde, *Oeuvres de Rigord et de Guillaume le Breton*, 1: 323.

no place in William's traditional panegyric, nor were they noticed by other chroniclers. The chroniclers took note of the most active and responsible of the king's ministers only after they had long been in power. Yet Philip Augustus and his ministers refashioned the government in ways comparable to those of Henry I and his *curiales*.⁷³

THE TRADITIONAL REGIME THAT PHILIP INHERITED included the distinctive features of an ambulatory court and fixed local officials. Like all medieval kings, Philip was constantly on the move accompanied by his entourage. The life of the tent, the wagon, and the packhorse was prompted by his need to fight battles, to administer justice, to supervise his domain, to be seen by his subjects, and sometimes even to be fed. Philip drew up lists of one hundred towns, bishoprics, abbeys, and manors that owed him annual hospitality. While the greatest number were concentrated in the old domain, these sites extended north to Saint-Riquier and Arras, west to Tours, south to Saint-Pourçain and Clermont, and east to Châlons-sur-Marne.⁷⁴ Whether Philip actually visited every locality on the list each year or occasionally accepted money in place of the visit (a dignified form of blackmail), he regularly circulated throughout his domain. No surviving document comparable to the English *Constitutio domus regis* describes the duties and wages of the French royal household, but the witness lists of royal charters show that the household, following the Carolingians, had the five traditional officers: the seneschal, the chamberlain, the butler, the constable, and the chancellor.⁷⁵ Originally appointed to perform domestic duties, the household officers had assumed governmental responsibilities long before Philip's reign. By the late twelfth century, however, these offices had become largely honorific and overshadowed by the king's swelling entourage, which, like Henry's, included members of the royal family, great barons and prelates on business with the court, lords and ecclesiastics from the vicinity through which the court was passing, and a great host of lesser figures who were designated simply as knights of the king (*milites regis*) and clerks of the king (*clerici regis*).

While the names and functions of Henry's entourage can be reconstructed from attestations to royal charters, those of Philip's court cannot be established with equal precision because his charters had become formalized and were attested only by the traditional five household officers whose names obscured all others attending court. Other evidence, however, suggests that the main work of Philip's court was performed by lesser men, especially the

⁷³ For the principal work on Philip Augustus, based almost exclusively on chronicle sources, see Alexander Cartellieri, *Philipp II. August, König von Frankreich*, 5 vols. in 4 (Leipzig, 1899–1922). For an early study of Philip's government, see Williston Walker, *On the Increase of Royal Power in France under Philip Augustus, 1179–1223* (Leipzig, 1888).

⁷⁴ Register A, Vatican, Ottoboni Lat. MS 2796, ff. 78v–79r; and Register E, Archives Nationales, Paris [hereafter AN], MS JJ 26, f. 302v. On the royal right to hospitality in general, see Carlrichard Brühl, *Fodrum, gistum, servitium regis* (Graz, 1968); and, for the Capetians specifically, see *ibid.*, 240–309.

⁷⁵ Léopold Delisle, *Catalogue des actes de Philippe Auguste* (Paris, 1856), lxxviii–lxxxviii.

chamberlains and the chancery clerks. In contrast to the now-honorific chamberlain of the royal household, these lesser chamberlains were actively engaged in royal affairs. Since they served in the king's bedchamber where valuables and treasures were kept, they often undertook financial duties, particularly the management of the royal domain.⁷⁶ When Philip Augustus allowed the household office of the chancellor to fall vacant in 1185, lesser clerks of the chancery carried on its essential work. Analyzing the handwriting of the French chancery as T. A. M. Bishop did for its English counterpart, the paleographer Françoise Gasparri has been able to distinguish seventeen different scribes during the forty-three years of Philip's reign, of whom no more than five or six worked simultaneously.⁷⁷ The French chancery was comparable in size to that of Henry I, and it produced the diplomata, charters, letters, and other documents essential to keep the government in operation. Within the chancery and the chamber, however, there is little evidence of definition of functions, specialization of tasks, or subordination of command. Again like the English, the French court consisted of a sizable but unstructured group of men around the king who did his bidding and sought his favor.

The other constant feature of the English and French monarchies was the local official rooted in his jurisdictional territory. Like the Norman vicomtes and English sheriffs of Henry I, the prévôts of Philip Augustus were well established in the Capetian royal domain by the twelfth century.⁷⁸ Following a medieval reluctance to discard any established institution, these officials remained at the foundation of local administration despite the addition of new agents. The prévôt was primarily a domanial agent, responsible for collecting the royal domain's produce and rents to which he added monopolies, tolls, exactions, and other rights productive of income. All domanial revenue was paid as a farm—that is, as an annual, fixed sum that was due the king regardless of the actual amount the prévôt collected. The farms were usually set low in order to allow the domanial agent a profit and to protect him against unfavorable years. Although not an efficient method for exploiting the domain, the system did ensure a fixed and calculable income for the king. In addition to these financial duties, the prévôts also had public responsibilities: garrisoning the castles, summoning the feudal levies, executing royal commands, and, most importantly, administering justice. In the prévôt's court, men of the region could plead their cases; this service the king was only too

⁷⁶ Philip's lesser chamberlains included Walter the Chamberlain and his two sons, Ursio and Walter the Younger; Peter, Gervais, and John from the Tristan family; and other minor figures, such as Renaud and Odo. For their financial functions, see page 894, below.

⁷⁷ Gasparri, *L'écriture des actes de Louis VI, Louis VII, et Philippe Auguste* (Geneva, 1973), 73–78. Philip's chancery had achieved such standardization that the different writing hands may be considered as types, thus obscuring individual scribes. On this point, see W. Prevenier, "L'écriture des actes des rois de France de 1108 à 1223, à propos d'un ouvrage récent," *Revue belge de philologie et d'histoire*, 54 (1976): 518–19.

⁷⁸ Achille Luchaire, *Histoire des institutions monarchiques de la France sous les premiers Capétiens (987–1180)*, 1 (2d ed., Geneva, 1891): 225–34; and Henri Gravier, "Essai sur les prévôts royaux du XI^e au XIV^e siècle," *Nouvelle revue historique de droit français et étranger*, 27 (1903): 539–74, 648–72, 806–74.

glad to offer because of the fines, confiscations, and other “profits of justice” it produced.

To keep track of this domanial income, records were required. Although the information about early Capetian records is not as clear as that about Anglo-Norman records, we presume that Philip Augustus received documents from his predecessors similar to Henry I’s records of shire farms and other local assessments. This presumption receives confirmation in Philip’s embarrassing loss of his baggage to King Richard I in the famous ambush at Fréteval in 1194. Philip was relieved not only of great treasure (about which medieval chroniclers loved to embroider the details) but also of certain important documents. According to William the Breton the latter included fiscal accounts, domanial charters (*scripta tributorum fisciue cyrographa*), inventories of payments (*tributa*), rents (*census*), tolls (*vectigalia*), and other domanial records.⁷⁹ Moreover, that a chamberlain, Walter the Younger, was charged with reconstituting the lost records suggests that they had originally emanated from the royal chamber. Inasmuch as these documents were products of a domanial economy, long since in operation, there is little reason to doubt that such records predated Philip’s reign.

Since a court constantly moving about in wagons was hardly suitable for safekeeping hoards of money, the treasury was the first institution of royal government to become stabilized and protected in strong castles. Like Henry, Philip inherited an established treasury from his predecessors. As early as 1146 the Capetians deposited their wealth at the tower of the Templars outside the walls to the north of Paris.⁸⁰ When the Knights Templars began to operate as bankers after the Second Crusade, the French kings were able to take advantage of these facilities not only to safeguard their treasure but also to transfer funds wherever needed. One Knight Templar, Brother Haimard, served as Philip’s treasurer throughout the latter half of the reign.

Because the underlying features of inherited government were fixed local agents and an ambulatory court, the basic flow of government among the early Anglo-Normans and Capetians was from the court to the countryside. The king and his entourage traveled incessantly throughout the realm to hear pleas, inspect the domain, and receive revenue. As long as the realm was small and delimited, the system was workable. Just as the conquest of Normandy accelerated the tempo of Henry’s movements after 1106, the acquisition of Vermandois, Picardy, Artois, and Normandy made increasing demands on Philip. For both kings, therefore, rapid territorial expansion required new or augmented governmental machinery. There was, however, one essential difference between the two realms: the Channel—not a negligible body of water—divided Henry’s lands; no such separation split Philip’s. Although C. Warren

⁷⁹ Henri-François Delaborde, Charles Petit-Dutaillis, and Jacques Boussard, eds., *Recueil des actes de Philippe Auguste, roi de France*, 3 vols. (Paris, 1916–66), 1: vi–vii, 2: vi–ix; and William the Breton, *Philippidos libri XII*, in Delaborde, *Oeuvres de Rigord et de Guillaume le Breton*, 2: 118–21.

⁸⁰ Léopold Delisle, “Mémoire sur les opérations financières des Templiers,” in *Mémoires de l’Institut National de France, Académie des Inscriptions et Belle-Lettres*, 33, pt. 2 (Paris, 1889): 1–64.

Hollister is doubtless correct in emphasizing that Henry considered England and Normandy to be one *regnum*, a real and practical division nonetheless separated the two parts.⁸¹ When Henry visited one part, he was necessarily away from the other, and this absence stimulated the growth of bureaucratic machinery and record-keeping. Governmental continuity required records and written communication with the absent king. By contrast, the French king rarely left his kingdom except to participate in crusades. Yet this brief exception illustrates the rule: when Philip left in 1190 for eighteen months on the Third Crusade, he issued an ordinance for governing the domain during his absence.⁸² And this document contains the first extant, comprehensive view of Capetian government and provides the earliest evidence for the emergence of bureaucratic machinery.

AN EXPANDED DOMAIN LED PHILIP AUGUSTUS to produce four innovations, matching those of Henry I: fixed archives, a financial court of audit, traveling justices, and a viceregency. That the early Capetians practiced some rudimentary form of record-keeping is indicated by the losses at Fréteval, but no evidence suggests how such records were preserved beyond their transport in the royal baggage. Louis VII may have kept some documents at the Parisian abbey of Saint-Victor, because of the close relationship between the royal court and that house. Louis did give his incoming letters to his retiring chancellor, Hugh of Champfleuri, who had them copied into a book kept at Saint-Victor. But the chancery did not regularly maintain its own records at the royal court, because the letter book of Saint-Victor was never a part of the royal archives.⁸³ What documents Philip actually lost in 1194 is debatable, but it is certain that after that very date the king began systematically to collect his incoming charters and probably deposited them at the royal palace at Paris, as Henry I did at Winchester. These archives constitute the origin of the Trésor des Chartes, a collection that survives to this day, which was named after the building in which it was later housed.⁸⁴ Ten years after the establishment of these royal archives, a scribe in the French chancery copied into a book a series of documents of interest to the royal court. Because they were not placed in any particular order, other scribes added material wherever they found space. By 1212 the book had become so unwieldy that another

⁸¹ For the increased tempo of Henry I's movements, see John Le Patourel, *The Norman Empire* (Oxford, 1976), 124–25. For the unity of the Anglo-Norman *regnum*, see Hollister, "Normandy, France, and the Anglo-Norman Regnum," *Speculum*, 51 (1976): 202–42; and Le Patourel, *The Norman Empire*, 124.

⁸² Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345.

⁸³ Vatican, Reg. Lat. MS 179; Achille Luchaire, "Étude sur quelques manuscrits de Rome et de Paris," *Université de Paris, Bibliothèque de la Faculté des Lettres*, 8 (1899): 31–39; and Françoise Gasparri, "Manuscrit monastique ou registre de chancellerie? À propos d'un recueil épistolaire de l'abbaye de Saint-Victor," *Journal des savants* (1976), 131–40.

⁸⁴ That Philip began his collection in 1194 can be demonstrated by arranging the contents of the present Trésor des Chartes chronologically and eliminating the known posterior additions. Prior to 1194 no single year was represented by more than five pieces, and most by only one. In 1195 the number jumps to eighteen, in 1199 it rises to twenty-eight, and it sharply increases thereafter. Also see Henri-François Delaborde, ed., *Layette du Trésor des Chartes*, 5 (Paris, 1909): ii–xxvii.

scribe was commissioned to make a clean copy on which he superimposed a rudimentary organization of ten chapters. Again, the process was repeated to produce a third copy in 1220 divided into eighteen chapters.⁸⁵ These registers, as they were called, contained feudal inventories, series of inquests, special financial accounts, miscellaneous information, and, most importantly, copies of the king's outgoing charters. These last included only a small fraction of what the royal chancery produced and cannot be compared with the contemporary chancery enrollments in England or even the papal registers, both of which are much more complete. But the French chancery clerks did produce, in effect, three cartularies or handbooks of information useful for the operation of royal government.

A fixed depository of incoming charters, registers containing outgoing charters and related useful information, and, as we shall see, a new series of financial accounts—these three collections indicate an important transition in French royal administration. Since the affairs of government had become too profuse and complex to be entrusted to the memory of an ambulatory king and his immediate entourage, some systematic and permanent record was required. These documents in turn provide evidence of an emerging bureaucracy. That the initiation of royal registers coincided with the annexation of Normandy demonstrates that the royal domain had become too large to be managed under the old system. For the first time the French government made a conscious effort to collect and preserve its own records, and for the first time the modern historian can see the French royal government from the inside. Measured by governmental documentation, French history, it can be said, begins with Philip Augustus.

Certainly, the most precocious achievement of the Anglo-Norman court was a financial session of audit with systematic records. Almost a century after the emergence of the English exchequer, Philip Augustus ordered in the ordinance of 1190 that his revenues be brought to Paris three times a year, where they were then to be recorded by a scribe at the Temple.⁸⁶ The records produced by these audits were tragically obliterated by a great fire that destroyed the Chambre des Comptes in Paris on the night of October 26, 1737. Yet not all was lost. Ten years earlier an antiquarian-minded functionary had "borrowed" (as antiquarians often did in those days) one of the accounts from the year 1202/03 and printed it as an appendix to a study on fiefs.⁸⁷ Thus, the financial court of Philip Augustus can be seen through one account com-

⁸⁵ The three registers were named by Delisle, in his *Catalogue des actes de Philippe Auguste*, vi-xix: Register A, Vatican, Ottoboni Lat. MS 2796 (1204-12); Register C, AN, MS JJ 7 (1212-20); and Register E, AN, MS JJ 26 (1220 and following). Also see Delaborde, *Recueil des actes de Philippe Auguste*, 1: x-xl; and Françoise Gasparri, "Note sur le *Registrum Veterius*: Le plus ancien registre de la chancellerie de Philippe Auguste," *Mélanges de l'école française de Rome*, 83, no. 2 (1971): 363-88. With the collaboration of Françoise Gasparri and Michel Nortier, I am preparing an edition of the registers of Philip Augustus.

⁸⁶ Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345. For the precocity of the English, see Bryce Lyon and A. E. Verhulst, *Medieval Finance: A Comparison of Financial Institutions in Northwestern Europe* (Providence, R.I., 1967), 51-52, 80-81.

⁸⁷ [Nicholas] Brussel, *Nouvel examen de l'usage général des fiefs*, 2 (Paris, 1727): cxxxviii-cxxxix, reprinted in Ferdinand Lot and Robert Fawtier, eds., *Le premier budget de la monarchie française*, Bibliothèque de l'École des Hautes Études, Sciences historiques et philologiques, fascicule no. 259 (Paris, 1932).

parable to the famous, and equally isolated, pipe roll of 1130.⁸⁸ Both provide precious, if momentary, glimpses into the intimate workings of government and show that financial procedures under Philip Augustus were similar to those under Henry I. Like the English sheriffs and Norman vicomtes, the French prévôts of 1202/03 came to Paris at regular intervals to acknowledge their farms and deduct their expenses, all to be recorded in systematic accounts. Although the French court was not called an exchequer (derived from a checkered board), it must have functioned on the principle of the abacus as did the Anglo-Norman court, because the counting units were suitable to such calculations. Not only were the prévôts closely scrutinized by the central court, but they came in closer contact with each other and the animating influence of the central government. The prévôts' triannual visits represented an important reversal in the flow of governmental activities: the country now came to the court, which was itself increasingly fixed at Paris.

In order to strengthen the bonds between the central court and the expanding country, Henry I employed itinerant justices and even itinerant sheriffs to greater effect. As early as 1184 Philip Augustus also experimented with a new official called the bailli. Again, the ordinance of 1190 affords the first clear view of this royal agent, who appears primarily as a judicial officer.⁸⁹ The bailli was instructed to hold periodic assizes in the domain and to report, like the prévôts, three times a year to Paris. These assizes extended to inhabitants of the royal domain the benefits of royal justice on a regular basis. The ordinance also commanded the baillis to take note of the judicial fines owed to the king. Accordingly, about a dozen baillis reported large sums for *expleta* or justice in the triannual accounts of 1202/03. In addition to the judicial revenues, the baillis acknowledged important receipts from forests, regalian rights over churches, scutage, coinage, and arbitrary taxes called *tailles*.⁹⁰ In effect, while the prévôts were responsible for the farms (that is, the regular domanial income), the baillis collected the occasional income that varied from year to year. In 1202/03 the revenues in the baillis' accounts totalled 65,000 *livres* and those in the prévôts' accounts, 31,000 *livres*.⁹¹

In addition to their judicial and financial duties, the baillis performed administrative tasks: they executed royal commands, supervised the prévôts, and held numerous inquests. The sworn inquest improved exploitation of the expanding royal domain by increasing knowledge of royal resources and rights. Royal agents employed this device to gain information in a region in which they were strangers. The baillis assembled a group of men from the

⁸⁸ Even if these two accounts had not survived by accident, we would still know of their existence from other references and could deduce their character from later examples.

⁸⁹ For the first appearance of the office, see Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 108; and, for the baillis in 1190, see *ibid.*, no. 345. The evidence for baillis under Louis VII is not yet convincing. For the single example, which is both too early and too vague, see Marcel Pacaut, *Louis VII et son royaume* (Paris, 1964), 177.

⁹⁰ For examples of the judicial revenues of the baillis, see Lot and Fawtier, *Le premier budget*, cxcix (1), CLXXIX (2); and, for other revenue, see *ibid.*, 54-79.

⁹¹ *Ibid.*, 48. These figures are only approximations, because the totals obscure complex procedures whereby the prévôts also accounted in the baillis' accounts.

neighborhood, placed them under oath, asked certain questions, and recorded the answers. Although the one hundred inquests that Philip Augustus compiled in his registers never matched the monumental achievement of the inquests in the English Domesday Books—an endeavor unsurpassed in the Middle Ages—the English and French inquests served similar purposes.⁹² Over two-thirds of Philip's inquests dealt with the resources of Vermandois, Artois, and Normandy—his newly acquired domains.

During 1202/03 roughly a dozen baillis were active according to the triennial accounts. A plot of the geographic location of their activities shows that they worked in three main areas out from Paris. Three worked in and around Paris and in the French Vexin toward Normandy. Five were active to the south, extending as far as Bourges and Auxerre to the southeast. And four worked to the northeast of Paris in Vermandois and Artois, extending to Laon in the east and Arras in the north. Although the direction of their activity can be discerned from the accounts, these “regions” were by no means discrete jurisdictions, because the baillis’ operations frequently interlaced. The charters that resulted from the judicial decisions and inquests of the baillis confirm this pattern and often show them working in teams of two or three. To take an example from the northeast, William Pastez collaborated with Peter de Béthisy and his brother Renaud de Béthisy as early as 1203. When Peter dropped out after 1205, he was replaced by Giles de Versailles by 1207. When William disappeared in 1215, his place was taken by Soibert de Laon. Renaud de Béthisy, the last of the original team, remained until 1221.⁹³ Unlike those of the later thirteenth century, the original baillis of Philip Augustus seldom took geographic titles; they were merely styled baillis of the king. The ordinance of 1190 announced that the king “was placing baillis in his lands that were distinguished by their own names”—meaning that the lands of the royal domain were to be distinguished and identified by the bailli.⁹⁴ This practice was precisely that found in the accounts of 1202/03.

Before the conquest of Normandy, therefore, Philip had devised traveling baillis for the royal domain who performed judicial, financial, and administrative functions comparable to those of the itinerant justices of Henry I in England and Normandy. In Flanders as well, local officials who exercised similar functions for the Flemish counts appeared in the late twelfth century and were designated by the titles of minister, justiciar, and prévôt.⁹⁵ But the

⁹² Although the original records of these inquests are scattered in Register A (Vatican, Ottoboni Lat. MS 2796) and in Register C (AN, MS JJ 7), they were collected in Register E (AN, MS JJ 26) and can be found there, principally between folios 124r and 285r.

⁹³ The baillis working in and around Paris and the Vexin were Robert de Meulan, Aleaume Hecelin, and Mathew Pisdœ; toward the south, Hugh de Gravelle, William de la Chapelle, Abelin, Guy Bernovin, and Terry de Corbeil; and toward the northeast, Renaud de Béthisy, Guy de Béthisy, Peter de Béthisy, and Nevelo the Marshal. For evidence of their operations, see Lot and Fawtier, *Le premier budget*. For evidence of their collaboration, see Léopold Delisle, “Chronologie des baillis et des sénéchaux royaux depuis les origines jusqu’à l’avènement de Philippe de Valois,” in Léopold Delisle, ed., *Recueil des historiens des Gaules et de la France*, 24 (Paris, 1904): *53–*58.

⁹⁴ Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345.

⁹⁵ Louis M. de Gryse, “Some Observations on the Origin of the Flemish Bailiff (Bailli): The Reign of Philip of Alsace,” *Viator: Medieval and Renaissance Studies*, 7 (1976): 243–94. One of Philip’s baillis, William Pastez, may have come from the same family that produced prévôts of Baupaume for Philip of Alsace, count of Flanders; see *ibid.*, 256, n. 75.

conquest of Normandy introduced complications. Philip inherited not only the duchy's itinerant justices but also a distinct local official (like the vicomte), also called a bailli. The Norman bailli, who first appeared under the Angevin dukes after the death of Henry I, concentrated on judicial functions, and Philip quite likely borrowed the term from Normandy even before the conquest. What distinguished the Norman bailli from the itinerant justices holding assizes throughout the duchy was the specific geographic jurisdiction of the former, called a baillage. These two Norman officials bring to mind the coexistence and intermingling of the English itinerant and shire justices under Henry I.⁹⁶ When Philip Augustus acquired Normandy in 1204, he adopted most of the Norman institutions intact. He retained, for example, the great Norman exchequer, which continued to operate biannually both as a bureau of audit and a court of law like its English counterpart.⁹⁷ Initially, he also retained both the Norman itinerant justices and the baillis, but shortly after the conquest the itinerant justices gave way to the baillis as the sole judicial officials stationed in the local baillages. By the end of the reign this Norman system of baillis presiding over baillages had superseded the collegiate and traveling baillis throughout the rest of the royal domain.⁹⁸

Like the prévôts, the French baillis took the road to Paris three times a year. Not only reporting on judicial assizes and rendering financial accounts, they also brought the king valuable information on the state of the royal domain. Now that the royal lands were too vast for the king's personal efforts at supervision, he relied increasingly on the baillis to keep him apprised. Once more, the processes of government were reversed to flow from the provinces to the center.

Henry I had found it necessary to create a viceregency in both England and Normandy because of his protracted absences on either side of the Channel. In France, however, the king was seldom absent from his realm. Suger, abbot of Saint-Denis, ran the kingdom when Louis VII was absent on the Second Crusade, and Philip's mother and uncle were the titular regents while the king was on the Third Crusade.⁹⁹ But, as the functions of finance and justice expanded and the administration of the realm became more bureaucratic, Philip also needed men at his court to run the more sophisticated machinery. Although the French chroniclers were normally descreet about influential members within Philip's court, this silence was finally broken in the latter half

⁹⁶ See pages 882–83, above.

⁹⁷ For the judicial records of the Norman exchequer, see Léopold Delisle, ed., *Recueil de jugements de l'échiquier de Normandie au XIII^e siècle (1207–1270)* (Paris, 1864).

⁹⁸ For the Norman baillis, see Charles H. Haskins, *Norman Institutions* (Cambridge, Mass., 1918), 151–52, 182–86; Sidney R. Packard, "The Judicial Organization of Normandy, 1189–1204," *Quarterly Law Review*, 40 (1924): 461–63; and Joseph R. Strayer, *The Administration of Normandy under Saint Louis* (Cambridge, Mass., 1932), 7–9, 19–22. For the importance of the shift from traveling, collegial baillis to stationary, single baillis, see James W. Fesler, "French Field Administration: The Beginnings," *Comparative Studies in Society and History*, 5, (1962–63): 82–87. More work is needed on the evolution of the Norman baillis and their adoption by the French. Although this study is not primarily concerned with the sources of influence, Normandy—both before and after its acquisition—undoubtedly exercised an important influence on Philip's government. For a magisterial study, see F. M. Powicke, *The Loss of Normandy, 1189–1240: Studies in the History of the Angevin Empire* (2d ed., Manchester, 1961).

⁹⁹ Pacaut, *Louis VII et son royaume*, 49–50, 55–59; Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345; and Cartellieri, *Philipp II. August*, 2: 105–06.

of the reign. While recounting the events of 1213 on the eve of the battle of Bouvines, the northern chronicles—the *Histoire des ducs de Normandie* and the *Anonymus of Bethune*—revealed for the first time that the inner council of the king comprised four men: the chamberlain, Henry Clément, Bartholomew de Roye, and Brother Guérin.¹⁰⁰ Yet these names can be found in the governmental documents since the establishment of the royal archives in the 1190s. Although the royal court contained other men known through governmental sources, these four formed an inner circle. The first figure was undoubtedly Walter the Younger, son of Walter the Chamberlain, whose family had long been familiar and trusted royal servants. Henry Clément and Bartholomew de Roye were both “knights of the king”—the former excelled in military service; the latter, in governmental affairs. The most noteworthy was Brother Guérin, a member of the Knights Hospitalers and a “clerk of the king.”

As each new series of royal records was created, the governmental functions of Brother Guérin were more clearly delineated. Toward the beginning of 1202 the royal charters, which earlier had ended with the formula, “the chancery being vacant,” now closed with a new phrase: “given by the hand of Brother Guérin.” This practice, which continued until 1210, indicates Guérin’s duties in the chancery. And as late as 1220 Guérin was the official who ordered the chancery clerks to compile the third and last of the royal registers.¹⁰¹ The financial account of 1202/03 reveals that Guérin was the king’s chief financial officer, who received and disbursed monies widely throughout the domain. The rolls of the account contain no other name that appears with more frequency or with larger sums of money.¹⁰² Five years later, when the Norman exchequer was re-established at Falaise and began producing its own records, Guérin was again placed in charge. Twice a year throughout the reign he journeyed to Falaise with Walter the Younger or Bartholomew de Roye to preside over the judicial sessions of the exchequer. In addition to these periodic duties, he frequently judged other cases and ordered numerous inquests.¹⁰³ Since he was present at all of the important judicial decisions, no one else was more closely associated with the work of the court.

From all appearances Brother Guérin was a new kind of official at the French court; he was not another Suger, who administered the realm in the king’s absences. Suger ceased his duties on the return of Louis VII, but Guérin served constantly and closely at the side of Philip Augustus. Active in the chancery, in finances, in justice, and in administration, this royal clerk performed all of the tasks essential to medieval government. Toward the end

¹⁰⁰ Fr. Michel, ed., *Histoire des ducs de Normandie et des rois d'Angleterre* (Paris, 1840), 120; and *Anonyme de Bethune*, in Delisle, *Recueil des historiens des Gaules et de la France*, 24: 764.

¹⁰¹ Petit-Dutaillis, *Recueil des actes de Philippe Auguste*, 2: no. 688; Boussard, *Recueil des actes de Philippe Auguste*, 3: no. 1120; and Delaborde, *Recueil des actes de Philippe Auguste*, 1: xxxi–xxxii.

¹⁰² See Lot and Fawtier, *Le premier budget*, index *sub nom.* Garinus. Bartholomew de Roye appears in four transactions, Walter and Younger in six, and Brother Guérin in over fifty, handling sums of money ranging from 45 *sous* to 1760 *livres*. Brother Haimard figured in every accounting because he was treasurer of the Temple.

¹⁰³ For his exchequer activities, see Delisle, *Recueil des jugements de l'échiquier*, 4–90. For some examples of his other judicial duties, see Boussard, *Recueil des actes de Philippe Auguste*, 3: no. 1378; and Delisle, *Catalogue des actes de Philippe Auguste*, nos. 1522, 1573, 1953, 1954, 2047, 2090, 2216.

of the reign as Philip grew older, Guérin increasingly acted in the king's place.¹⁰⁴ In 1213 he was elected to the bishopric of Senlis not far from Paris—undoubtedly at the instigation of the king. In that year—the same year, in fact, in which the northern chronicles mentioned him for the first time—the royal historian, William the Breton, described Guérin as “the special counselor of King Philip because of his wisdom in the royal hall and his incomparable gift of counsel . . . so that he handled the affairs of the kingdom and the needs of the churches as if he were second to the king.”¹⁰⁵ *Secundus a rege* was not an unusual title for important members of the king's court. Under the early Capetians it was accorded to Stephen of Garlande and under Henry I, to Roger of Salisbury.¹⁰⁶ Yet the title was remarkably appropriate to Guérin and Roger. Both acted “second to the king”—the former in his presence, the latter both in his presence and during his frequent absences.

TO STAFF THESE NEW INSTITUTIONS as well as the royal courts, medieval kings had at their disposal a wide choice of personnel ranging from great magnates, both lay and ecclesiastic, to lesser knights and clerics. Whether great or small, these men were called *curiales*, *consiliarii*, or, most frequently, *familiares* (from the royal *familia*), all terms reflecting close association with the king. The basic problem facing the king was to determine from which elements of this broad spectrum he should draw his most trusted familiars. The research of Jean-François Lemarignier and Eric Bournazel has elucidated the solutions of the early Capetians to this problem.¹⁰⁷ Louis VI and Louis VII, Philip's grandfather and father, drew heavily on lesser men—chamberlains, knights, clerics, and the like. The career of Suger, abbot of Saint-Denis, provides the most prominent example: although of humble origin, he rose to become the foremost counselor of Louis VI and Louis VII. In addition to such lesser men, the early Capetians also employed in household offices members of powerful castellan families from the Ile-de-France—for example, the Montlhérys, Rocheforts, La Tours, and Garlandes. When the Garlandes, in particular, monopolized household positions, Louis VI was forced to allow the offices to lie vacant to free himself of the family's usurpations. As Louis VII moderated castellan influence at his court, he began to attract the cooperation of great barons outside the royal domain during the second half of his reign. The influential post of seneschal was occupied by a royal cousin, Ralph, count of Vermandois, and later by Thibaut, count of Blois, from the powerful house of Champagne.¹⁰⁸ This close association with castellan and baronial families naturally generated competition and conflict.

¹⁰⁴ For examples, see Alexandre Teulet, ed., *Layettes du Trésor des Chartes*, 1 (Paris, 1863): no. 1572; and Delisle, “Chronologie des baillis et des sénéchaux royaux,” *288.

¹⁰⁵ William the Breton, *Gesta Philippi Augusti*, 1: 256–57.

¹⁰⁶ For Stephen of Garlande, see Léon Mirot, *La chronique de Morigny 1095–1152* (Paris, 1912), 33–34, 43; and, for Roger of Salisbury, see pages 876–78 and note 26, above.

¹⁰⁷ Lemarignier, *Le gouvernement royal aux premiers temps capétiens (987–1108)* (Paris, 1965); and Bournazel, *Le gouvernement capétien au XII^e siècle, 1108–1180* (Paris, 1975).

¹⁰⁸ Bournazel, *Le gouvernement capétien au XII^e siècle*, 23–27, 31–91, 111–15; and Pacaut, *Louis VII et son royaume*, 172–76.

A youth of fourteen at his accession in 1179, Philip Augustus fell heir to this legacy of baronial predominance. His coronation was overshadowed by the house of Flanders, represented by Count Philip, who carried his sword, and by the house of Champagne, represented by William, archbishop of Reims, who performed the consecration.¹⁰⁹ The king subsequently married Elizabeth of Hainaut, niece to the count of Flanders, and Archbishop William was brother to the king's mother, as was the royal seneschal, Count Thibaut of Blois. Thereafter, however, the house of Flanders lost ground to the house of Champagne. The archbishop of Reims accompanied the king at most of the recorded gatherings of the royal court. When the pope summoned the archbishop to Rome in 1184, Philip refused to allow William to depart, because he was the "vigilant eye of the king's counsels and the right hand of his affairs."¹¹⁰ When Philip himself departed on the Third Crusade in 1190, the regency naturally devolved to the archbishop and the queen mother of the Champagne house.

In the years following his return from the Holy Land, Philip instituted a gradual but significant change in the pattern of royal recruitment of officers, a change that had lasting effects on the nature and composition of the Capetian court. As early as 1191 Philip was determined to resist baronial influence at court and to inaugurate a new personnel policy. When the aged seneschal, Thibaut of Blois, succumbed at the siege of Acre in that year, the king left the office vacant, as he had the chancellorship in 1185.¹¹¹ Although Archbishop William lived until 1202, his prominence in Philip's government gave way to a new group of *familiars*, whose names appear in the emerging governmental documentation of the 1190s. The contemporary chroniclers took little notice of these new men because, unlike their predecessors, they were not of high rank. The four *familiars*, whom the northern chroniclers belatedly identified at the heart of royal government in 1213, were of lowly origins. The chamberlain, Walter the Younger, the son of Walter the Chamberlain, came from a family that originally possessed no toponymic. Among the royal knights, Henry Clément belonged to a minor family from Chateau-Landon, which in the past had supplied royal marshals for the court. Bartholomew de Roye was the younger son of a small knightly family in the Vermandois. And the origins of Brother Guérin were so obscure that they have not yet been uncovered.¹¹² The only castellan family reappearing under Philip was the Garlande, but its members played a minor rôle in contrast to their former prominence.¹¹³

The king handsomely rewarded these new and lowly men for their service. Walter the Chamberlain was endowed with pensions and liveries and was

¹⁰⁹ Cartellieri, *Philipp II. August*, 1: 41–55.

¹¹⁰ Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 109.

¹¹¹ Delisle, *Catalogue des actes de Philippe Auguste*, lxxxi–lxxxii, lxxxv–lxxxvi.

¹¹² E. Richemond, *Recherches généalogiques sur la famille des seigneurs de Nemours du XII^e au XV^e siècle*, 2 vols. (Fontainebleau, 1908), 2: 59–71, 1: 196–201; Pierre Daon, "Barthélemy de Roye, chambrier de France," in *Écoles des Chartes, Positions de thèses* (Paris, 1943), 49–54; and "Le chancelier Guérin," *Comité archéologique de Senlis: Comptes rendus et mémoires*, 3d ser., 2 (1887): 71–148.

¹¹³ William de Garlande, for example, was to stand by as a substitute in the administration of the kingdom during Philip's absence in 1190; Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345.

married to the heiress of Nemours, from whose possessions his family drew its patrimony.¹¹⁴ In addition to two sons who succeeded him as royal chamberlains, three other sons were provided with bishoprics, almost certainly at the king's suggestion.¹¹⁵ Bartholomew de Roye was enriched with gifts of clothing, money, houses at Paris, fiefs in Normandy, and, finally, a marriage to a daughter of the Norman count of Évreux. In 1208 the king conferred on him the chamberlainship of the royal household, but this position had become honorific and did not alter his service to the king.¹¹⁶ Brother Guérin received numerous properties in the southern domain, and his career was crowned by the bishopric of Senlis in 1213.¹¹⁷ Despite these royal favors, the king's new men were unable to advance their social status and attain castellan or baronial rank. Even the active and loyal service of Bartholomew de Roye did not benefit him or his descendants with baronial status, despite his brilliant marriage with the family of Évreux. The only exception was Guérin's elevation to episcopal rank; still, his promotion never diminished the frequency or effectiveness of his royal service.

The elevation of men of low station to positions of authority naturally increased the temptation of *familiares* to enrich themselves by illicit means. As in Henry I's administration, such activities occurred most frequently on the local level, where royal officials misused their powers to extort from the inhabitants of the royal domain. Nevelo the Marshal, for example, the northernmost bailli in Artois, established a reputation for rapacious exactions that was amply confirmed by later investigations under Louis IX. Cadoc, the celebrated mercenary captain turned Norman bailli, could not repress his buccaneering habits and so oppressed his subjects that he was eventually removed.¹¹⁸ At the end of the reign an *exemplum* circulated about a bailli of Philip Augustus who defrauded a widow of her vineyard and was caught and corrected by the king.¹¹⁹ Such stories reflected popular fears over the awesome powers invested in the royal officers that led to abuse.

Except for the case of Cadoc, Philip appears to have been satisfied with the services of these new *familiares* and baillis, a confidence that is best indicated by the length of time they remained in his employ. Walter the Younger, Bartholomew de Roye, and Brother Guérin all surfaced in the royal entourage in the 1190s. Although Walter died on a crusade in 1218, Bartholomew and

¹¹⁴ Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 372; Lot and Fawtier, *Le premier budget*, CXLVI (1); and Richemond, *Recherches généalogiques sur la famille des seigneurs de Nemours*, 1: 22.

¹¹⁵ For the observation of Alberic of Trois-Fontaines, see his *Chronica*, ed. Paul Scheffer-Boichorst, in *Monumenta Germaniae Historica, Scriptores*, 23 (Hannover, 1874): 884.

¹¹⁶ Petit-Dutaillis, *Recueil des actes de Philippe Auguste*, 2: nos. 542, 583A, 745, 886, 959; Lot and Fawtier, *Le premier budget*, CLVI (1), CXCIV (2), CCIII (2), CLXIV (1); Daon, "Barthélemy de Roye," 49–50; and Delisle, *Catalogue des actes de Philippe Auguste*, lxxxiii.

¹¹⁷ Lot and Fawtier, *Le premier budget*, CXLII (2), CXCI (1), CXCIII (1, 2); and see pages 900–01, above.

¹¹⁸ For Nevelo the Marshal, see Gerald of Wales, *De jure et statu Menevensis ecclesie*, in his *Opera*, ed. J. S. Brewer, vol. 3, Rolls Series (London, 1863): 240; and *Anonyme de Bethune*, 254, 770. For Cadoc, see Delisle, "Chronologie des baillis et des sénéchaux royaux," *130–*33.

¹¹⁹ Richer de Sénonès, *Gesta Senoniensis ecclesie*, ed. G. Waitz, in *Monumenta Germaniae Historica, Scriptores*, 25 (Hannover, 1880): 288–90; and Ralph of Coggeshal, *Chronicon*, ed. J. Stevenson, Rolls Series (London,

Guérin outlived the king and were designated executors of his testament in 1222.¹²⁰ Since they furnished up to thirty years of service, they must have been young men when they joined the royal court. Fifteen to twenty years were not, in fact, unusual for royal baillis. Lacking substance and position of their own, such "new" young men were completely dependent on the king for their wealth and status. For this reason, their loyalty and effectiveness were generally beyond reproach.

These "new men" constitute another important shift in the reign of Philip Augustus. To be sure, Walter the Younger, Bartholomew de Roye, and Brother Guérin found their predecessors in the chamberlains, knights, and clerks of Philip's father and grandfather. But Louis VI and Louis VII had also relied heavily on the important castellan and baronial elements in their entourage for advice and cooperation, although the kings had often quarreled with them. After the death of his uncle, Archbishop William of Reims, Philip outgrew the baronial influence of the Champagne party and decided to strike out on his own, aided by men totally dependent upon his favor. In effect, Brother Guérin replaced William of Champagne as the chief counselor of the king. After the expulsion of the English, the untroubled operation of royal government was sufficient evidence of the success of this policy.

PHILIP'S EFFORTS TO EXCLUDE THE CASTELLANS AND BARONS from his government and to rely more heavily on "new men" was not only a decisive departure from past Capetian policy, but also a contrast to the policy of Henry I. Herein lies an important difference between the entourages of the English and the French kings. The *curiales* of Henry I comprised both lesser men—like the Bassets and the Clintons—and magnates—like the Bigods and William of Warenne—who were equally rewarded with favors. In addition to the breadth of royal patronage, feudal tenure also accounted for the divergent practices. Feudal tenure in England was so complex that in any one county the great baronial holdings and the royal domains were closely enmeshed. C. Warren Hollister has shown that this landholding pattern enabled the king to manipulate the magnates and to draw them into his court. On the other hand, the fiefs of the great French barons were normally distinct from those of the king, thus permitting them greater independence from the royal court. Whereas the English kings included the great magnates among the *curiales*, the French kings could exclude them. The former produced a large entourage, the latter a small court.

This distinctive French trait was noticed as early as 1200, when the poet Giles of Paris complained that Philip Augustus rarely took counsel from anyone except the very few whom he tolerated at court.¹²¹ This disparity with English practice became more pronounced by the end of the reign. In 1227 a

¹²⁰ Teulet, *Layettes du Trésor des Chartes*, 1: no. 1546.

¹²¹ M. L. Colker, "The 'Karolinus' of Egidius Parisiensis," *Traditio*, 29 (1973): 306.

citizen of Caen sent intelligence to Henry III of England of a conversation he had overheard between the castellan of Caen and Master Nicholas, the clerk of Brother Guérin. The two Frenchmen contended that the English kings did not have the good sense of the French in taking counsel. Whereas King Philip consulted Brother Guérin and Bartholomew de Roye, and only these two, the English kings took counsel from a great number. When the English kings wished to declare war, therefore, their intentions were publicized even before the decision was taken.¹²² Behind this practical disadvantage lay the more fundamental difference between France and England introduced by Philip Augustus: the French king's *curiales* consisted only of a small number of his creatures; the English *curiales* included a large number of great barons. Whether the one choice led to Louis IX, Philip the Fair, and royal absolutism and the other to Magna Carta and constitutional monarchy is a question worthy of speculation.

¹²² Pierre Chaplais, *Diplomatic Documents Preserved in the Public Record Office* (London, 1964), 139-40.

Anticipatory Association of the Heir in Early Capetian France

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MODERN HISTORIANS HAVE TRADITIONALLY VIEWED ROYAL SUCCESSION in early Capetian France as a conflict between hereditary and elective principles. Although the operation of these principles can be traced elsewhere in medieval Europe, especially in Germany, they have had a particular interest for France, where the dispossession of the last Carolingians was followed by the reign of a line of kings who lacked dynastic legitimacy. The essential means by which the early Capetians were seen to have kept the throne in their family was through the association of the eldest surviving son in the royalty during the father's lifetime. By this maneuver the elder king could control the proceedings and so extend the rule by his line. In 987, the first year of his reign, Hugh Capet thus thwarted the designs of those who wished for an elective monarchy by having his son Robert consecrated and crowned king. Robert in turn associated his eldest son and, upon the premature death of the first son, had his second son anointed and crowned. Succeeding kings continued the practice until 1179, when Philip Augustus was made king with the ailing Louis VII. The practice has been seen as a reflection of the weak hereditary title of the dynasty until the reign of Philip Augustus, who abandoned the custom. The Capetians have been viewed as truly hereditary monarchs only from the time when they were able to discontinue this practice in favor of the automatic or quasi-automatic succession of the son upon his father's death.¹

Comparison with other medieval European monarchies has seemed to con-

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¹ This interpretation has dominated modern scholarship since the publication of Achille Luchaire's classic works; see his *Histoire des institutions monarchiques de la France sous les premiers Capétiens, 987-1180*, 1 (Paris, 1883): 57-83. Luchaire's thesis remained virtually unchanged in the second edition; see *ibid.*, 1 (2d ed., Paris, 1891): 60-87. Luchaire's presentation formed the core for Percy Ernst Schramm, *Der König von Frankreich: Das Wesen der Monarchie vom 9. zum 16. Jahrhundert*, 1 (2d ed., rev., Darmstadt, 1960): 97-111 (these pages are little altered from the first edition of 1939). His thesis was also the framework used by many other scholars; in particular, see Charles Petit-Dutaillis, *La monarchie féodale en France et en Angleterre (X^e-XIII^e)*

firm this interpretation. Five German emperors of the tenth and eleventh centuries, then four Hohenstaufen, insecure in their hereditary titles, had their sons elected or crowned king during their own lifetimes. One of them, Otto II, was even made emperor with his father, and only the pope's refusal to perform the coronation ceremony blocked a similar plan by Frederick Barbarossa.² Henry I of England, faced with the Norman claims of his nephew, William Clito, had his son William Adelin named king-designate; and Stephen, for analogous reasons, attempted to have his own eldest son crowned.³ Whether Henry II of England, whose hereditary claim was undisputed after 1153, acted on similar motives in effecting the coronation of his son, the young Henry, is a separate question.⁴ The recurrence of the same practice in these monarchies has tended to demonstrate that anticipatory association of the heir was a royal device to ensure hereditary rule in the face of opposition to it.⁵ Most scholars have treated the measure as a kind of touchstone and have largely discounted evidence that the Capetian throne was hereditary before the reign of Philip Augustus on the grounds that the Capetians must have lacked dynastic legitimacy if they still found it necessary to associate their heirs.⁶

siècles) (Paris, 1933), trans. by E. D. Hunt as *The Feudal Monarchy in France and England from the Tenth to the Thirteenth Centuries* (London, 1936), 27–30, 311; Robert Fawtier, *Les Capétiens et la France: Leur rôle dans sa construction* (Paris, 1942), trans. by Lionel Butler and R. J. Adam as *The Capetian Kings of France: Monarchy & Nation (987–1328)* (London, 1960), 48–58; and Ferdinand Lot and R. Fawtier, *Histoire des institutions françaises au Moyen Âge*, vol. 2: *Institutions royales* (Paris, 1958), 12–16.

² There were eleven such anticipatory associations, even if Otto I's designation by the dying Henry I in 936 is not included: Otto II as king in 961 and as emperor in 967 with Otto I; Otto III as king in 983 with Otto II; Henry III with Conrad II in 1028; Henry IV with Henry III in 1053; Conrad and Henry V, sons of Henry IV, with the latter in 1087 and 1098–99; Henry, son of Conrad III, with his father in 1147; Henry VI as king in 1169 with Frederick I, and Frederick's plan to make Henry co-emperor; Frederick II in 1196 with Henry VI; and Henry and Conrad, sons of Frederick II, with their father in 1220 and 1227. See Georg Waitz, *Jahrbücher des Deutschen Reichs unter König Heinrich I.* (Leipzig, 1885), 172–73; Rudolf Köpke and Ernst Dümmler, *Kaiser Otto der Grosse* (Leipzig, 1876), 322, 429; Karl and Mathilde Uhlirz, *Jahrbücher des Deutschen Reiches unter Otto II. und Otto III.*, 2 (Berlin, 1954): 6–9; Ernst Steindorff, *Jahrbücher des Deutschen Reichs unter Heinrich III.*, 2 vols. (Leipzig, 1874–81), 1: 4, 2: 227–28; Gerold Meyer von Kronau, *Jahrbücher des Deutschen Reiches unter Heinrich IV. und Heinrich V.*, 7 vols. (Leipzig, 1890–1909), 1: 8–9, 4: 160–61, 5: 26–27, 57; Wilhelm Bernhardt, *Könrad III.*, 2 (Leipzig, 1883): 546–47, 558; Theodor Toeche, *Kaiser Heinrich VI.* (Leipzig, 1867), 27, 41, 435–36, 443–46, 513–27; and Thomas C. Van Cleave, *The Emperor Frederick II of Hohenstaufen: "Immutator Mundi"* (Oxford, 1972), 116, 123–24, 402.

³ For the association of William Adelin with Henry I in 1119 and 1120, see C. Warren Hollister, "Normandy, France, and the Anglo-Norman Regnum," *Speculum*, 51 (1976): 224–29. On Stephen's efforts for his eldest son, Eustace, in 1152, see R. H. C. Davis, *King Stephen, 1135–1154* (Berkeley, 1967), 117.

⁴ In 1155, Henry II had the English barons swear fealty to his eldest son, William, as heir to the throne and swear also to make his second son, Henry, king if William died prematurely. But this measure was taken at the beginning of the reign, when Henry could have feared that his brothers might dispute the rights of his sons in the event of his own early death. The association of the young Henry in 1170 is best understood as part of the partition of the succession to Henry II and Eleanor that Henry II arranged in 1169. Robert of Torigny, *Chronicle*, in Richard Howlett, ed., *Chronicles of the Reigns of Stephen, Henry II, and Richard I*, 4, Rolls Series, no. 82 (London, 1889): 184, 240–41, 245; and W. L. Warren, *Henry II* (Berkeley and Los Angeles, 1973), 108–11.

⁵ Although the sketch of monarchical comparisons given here is more detailed than those found generally in the scholarship, such comparison has been a premise in many studies. For citation of Carolingian, Ottonian, and other monarchical precedents, see Schramm, *König von Frankreich*, 81, 106–07, 109, 111; Lot and Fawtier, *Institutions royales*, 14; and Jan Dhondt, "Élection et hérédité sous les Carolingiens et les premiers Capétiens," *Revue belge de philologie et d'histoire*, 18 (1939): 935. For citations by English historians of Capetian precedents for the anticipatory association of the young Henry, see G. O. Sayles, *Medieval Foundations of England* (rev. ed., New York, 1961), 349; and Warren, *Henry II*, 110–11.

⁶ Luchaire, *Histoire des institutions monarchiques*, 1 (2d ed.): 87; Petit-Dutaillis, *Feudal Monarchy in France and England*, 311; and Lot and Fawtier, *Institutions royales*, 15.

ON CLOSER EXAMINATION, however, there are strong reasons to distrust this interpretation, which was based on comparisons of monarchical practice. First, the monarchies so compared were conspicuously different from one another, as were the circumstances of their successions. In Germany, the changes of dynasty and the known opposition of the princes and, often, of the papacy to hereditary rule in the German kingdom make that case unique, at least from the time of the investiture controversy onward.⁷ In England, Henry I and Stephen wanted to associate their sons because of challenges to their rule by members of their own families who had superior hereditary claims.⁸ In France, after the very first years of Capetian rule, similar situations did not arise. When the rule of certain Capetians was challenged, it was by the kings' younger brothers, who were dissatisfied with their station or whose alleged rights to the throne were advanced by barons seeking their own gain. Rule by the dynasty itself was not contested.⁹

Second, not all Capetian cases of anticipatory association had the same purpose. The cases are, in fact, highly individual in character. The association of Robert II with Hugh Capet appears clearly as a maneuver to assure Robert's succession to the throne, despite the lack of hereditary right. Hugh proposed Robert's consecration on the pretext that, since he intended to go to Catalonia to aid the count of Barcelona against a Muslim invasion, two kings were needed to maintain stability if one of them was killed on the expedition. Although there was some opposition to the plan, Hugh's will prevailed and Robert was crowned; but the expedition to Spain never occurred.¹⁰ The situation was different a generation later, when Robert had his own sons crowned. No one contested the principle that he should be succeeded by one of his sons, though there was intense dispute over which of them should be chosen. The eldest son, Hugh, was associated without difficulty, but after his death, when the king proposed to have his second son, Henry, consecrated, the queen and certain magnates protested that the third son was better

⁷ For a synthesis of the German situation, see Geoffrey Barraclough, *The Origins of Modern Germany* (rev. ed., New York, 1963), 115, 120–21, 154–59, 162, 200–04.

⁸ It may well be that, since Robert Curthose had never succeeded in making himself king of England, the claims of his son, William Clito, to that kingdom were weaker than those of William Adelin, who was the son of the reigning monarch; in contrast, Clito's rights in Normandy, of which his father was hereditary duke and in which Henry I's status was undefined, were undeniably strong. In practice, the two successions were interrelated and, with the exception of the single element of the homage to the French king for Normandy, were so treated by Henry I. See Hollister, "Normandy, France, and the Anglo-Norman Regnum," 214–20, 224–33; Charles W. David, *Robert Curthose, Duke of Normandy* (Cambridge, Mass., 1920), 181–85; and William of Malmesbury, *De gestis regum Anglorum*, ed. William Stubbs, 2, Rolls Series, no. 90 (London, 1889): 495.

⁹ For a fine treatment of this point, see Dhondt, "Élection et hérédité sous les Carolingiens et les premiers Capétiens," 951–52. I discount Suger's charge, written sixty years after the alleged event, that in the early 1080s the count of Corbeil had boasted that he would make himself king; the count did not have the resources to realize such a scheme. Suger also stated that William II of England aspired to the French throne. Suger, *Vie de Louis VI le Gros*, ed. and trans. Henri Waquet (Paris, 1929), 150, 10. Neither of Suger's claims carries conviction; both are probably attributable to his desire for drama in his account.

¹⁰ Richer, *Histoire de France (888–995)*, ed. and trans. Robert Latouche, 2 (Paris, 1937): 164–66, nn.; and Dhondt, "Élection et hérédité sous les Carolingiens et les premiers Capétiens," 936–37. Also see Ferdinand Lot, *Les derniers Carolingiens: Lothaire, Louis V, et Charles de Lorraine* (Paris, 1891), 216–17, 220–21, 163. I credit Hugh with less sincerity in this proposal than does Lot; it would have been reckless for Hugh to have journeyed to Spain before resolution of the challenge for the throne that was certain to come from the Carolingian Charles of Lorraine.

qualified; only after much controversy was the king able to impose his own choice.¹¹ In the third generation, Henry I had his eldest son, Philip, consecrated under still different circumstances. Henry was planning to invade Normandy and, not surprisingly, wished to have his seven-year-old heir crowned before this extremely dangerous campaign. Given the lack of sources, it is impossible to determine whether Henry was concerned over his son's actual right to the throne or only desired to strengthen Philip's position in expectation of what in that society would almost necessarily be a troubled minority.¹²

The instances of royal association from the twelfth century add further diversity. Louis VI was "designated"—but not crowned—king during his father's lifetime.¹³ Louis VI's eldest son, Philip, was designated king as a small child but was consecrated only nine years later, after Louis had re-asserted control over the royal government through the ouster of the overpowerful Garlande family from the great offices of his household; the association thus appears as an act of consolidation of the father's strengthened position.¹⁴ The last case of Capetian anticipatory association, that of Philip Augustus in 1179, was occasioned by a crisis in the health of Louis VII that rendered the old king unable to rule.¹⁵ From birth Philip had been proclaimed heir to the throne and was widely accepted as such. Although his association may have prevented interregnal disturbances at his father's death—that is, breaches of the peace due to the temporary lack of royal enforcement—the evidence will not really permit the reading that his right to the throne was in question.¹⁶

Of the examples of Capetian association, therefore, only the first, that of Robert II with Hugh Capet, clearly fits the traditional interpretation. The last, that of Philip Augustus with Louis VII, plainly does not. The intervening

¹¹ Dhondt, "Élection et hérédité sous les Carolingiens et les premiers Capétiens," 937–48. But note that Dhondt has followed the error of the editors of the *Recueil des historiens des Gaules et de la France* in attributing one of the essential documents to Odoric, bishop of Orléans; the writer was almost certainly Hildegard of Poitiers. For the error, see *HF*, 10: 504; and, for the correction, see Christian Pfister, *Études sur le règne de Robert le Pieux (996–1031)* (Paris, 1885), 76 n. 1.

¹² For the context for this consecration, see Jan Dhondt, "Les relations entre la France et la Normandie sous Henri I^{er}," *Normannia*, 12 (1939): 465–86. For the coronation itself, see the protocol from it in *HF*, 11: 32–33. On the minority of Philip I, see Maurice Prou, ed., *Recueil des actes de Philippe I^{er}, roi de France (1059–1108)* (Paris, 1908), xxviii–xxxii; Eugène de Certain, ed., *Les Miracles de saint Benoît* (Paris, 1858), 314; and the archbishop of Reims to the pope, in *HF*, 11: 498. In his letter the archbishop expressed his fear that without the king the unrestrained quarrelling of the barons would bring about the ruin of the kingdom.

¹³ Augustin Fliche, *Le règne de Philippe I^{er}, roi de France (1060–1108)* (Paris, 1912), 78–83; and Achille Luchaire, *Louis VI le Gros: Annales de sa vie et de son règne (1081–1137)* (Paris, 1890), nos. 8, 11, 16, 31, etc.

¹⁴ Léopold Delisle, "Sur la date de l'association de Philippe, fils de Louis le Gros, au gouvernement du royaume," *Journal des savants* (1898), 736–40; and Luchaire, *Louis VI le Gros*, xlix–liii, nos. 399, 420, 433. For such a motive for anticipatory association, note the advice to Louis VI after Philip's death that he make his second son, the future Louis VII, king "ad refellendum emulorum tumultum"; Suger, *Vie de Louis VI le Gros*, 268.

¹⁵ Rigord, *Gesta Philippi Augusti*, in Henri-François Delaborde, ed., *Oeuvres de Rigord et de Guillaume le Breton*, 2 vols. (Paris, 1882–85), 1: 9–10.

¹⁶ For a fuller presentation of this view, see my "Royal Succession in Capetian France: Studies of Familial Order and the State" (Ph.D. dissertation, Harvard University, 1973), chap. 2: "Philip Augustus as Heir to the Throne." For an inventory of most of the documents, see Alexander Cartellieri, *Philipp II. August, König von Frankreich*, 1 (Leipzig, 1899): "Beilagen," 49–61. On the disorders that customarily occurred at the deaths of kings, see Auguste Molinier, ed., *Vie de Louis le Gros par Suger suivie de l'histoire du roi Louis VII* (Paris, 1887), 147.

cases are obscure in many regards, but the applicability of the received explanation to them is problematical at best. In the absence of clear documentation to the contrary, scholars have generalized the significance of the first case to all other instances of the measure over the course of two centuries, as if its purpose had been the same each time. The implausibility of that premise for a society that was governed chiefly by custom is evident: it seems unlikely that people for whom property rights were established by the "long usage" of thirty or forty years would not willingly accept the Capetians as their royal dynasty after a reign of seventy or even one hundred years.¹⁷ If anticipatory association of the heir is taken as proof of such an anomalous interpretation, our appraisal of that custom should be reassessed.

The only constant element in the Capetian practice of anticipatory association was the father's wish or expectation that his son should succeed him on the throne. Comparison with other medieval European monarchies reveals little more than that German and Anglo-Norman royal fathers had the same wish or expectation and that some of them used this means to ensure their sons' successions. Nothing in the evidence requires that anticipatory association of the heir be seen necessarily as a sign of a weak hereditary title, and the example of Henry II of England and the young Henry strongly suggests that such was not always the case.¹⁸ Reference to Carolingian precedents offers no reliable guidance, because the circumstances of the monarchy when Charlemagne and Louis the Pious associated their sons were radically different from those of later generations. It is arguable, moreover, that the Carolingian cases are extraneous to the period under review, since the tenth- and eleventh-century rulers either were unaware of them or else understood such knowledge as they had in ways that can no longer be determined or properly assessed.¹⁹ A comparative survey of this usage among the medieval monarchies is thus of limited value.

¹⁷ Dhondt alone has argued that from the beginning the Capetians were secure in their control over the royal succession and thus were "la dynastie royale"; "Élection et hérité sous les Carolingiens et les premiers Capétiens," 951–53. Disproportionate attention has been paid to "anti-Capetian" or "legitimist" statements in some of the chronicles; see the review by Karl Ferdinand Werner, "Die Legitimität der Kapetinger und die Entstehung des 'Reditus regni Francorum ad stirpem Karoli,'" *Die Welt als Geschichte*, 12 (1952): 208–13. But see Gabrielle M. Spiegel, "The *Reditus Regni ad Stirpem Karoli Magni*: A New Look," *French Historical Studies*, 7 (1971): 153 n. 36; and my "Dynastic Structures and Capetian Throne-Right: The Views of Giles of Paris," *Traditio*, 33 (1977): 242, n. 77. Many writers repeated from their sources the charge that Hugh Capet was a usurper, but they seem to have regarded his successors as legitimate kings. In isolated cases the dynasty itself was condemned, but such instances appear far removed from the political realities; for example, a chronicle from the monastery of Evrière, at Angers, which styled the Capetians *pseudo-reges*, was written during the late 1050s, a period of firm alliance between Henry I and the count of Anjou; Paul Marchegay and Émile Mabilley, eds., *Chroniques des églises d'Anjou* (Paris, 1869), 163; and Olivier Guillot, *Le comte d'Anjou et son entourage au XI^e siècle*, 2 vols. (Paris, 1972), 1: 79–81, 89–91. Fulk Réchin of Anjou, ca. 1100, held the Capetians in low regard in comparison to the Carolingians, but he did not question the Capetian royal title; see the *Fragmentum Historiae Andegavensis auctore Fulcone Richin, comite Andegavorum*, in Paul Marchegay and André Salmon, eds., *Chroniques d'Anjou* (Paris, 1856), 376, 379, 381. Werner has hypothesized an increased interest in the Carolingians and, as a corollary, reproaches of dynastic illegitimacy against the Capetians in the twelfth century; "Die Legitimität der Kapetinger," 211–18. But Henry II's careful observance of his feudal obligations toward Louis VII provides better evidence of the attitude of the magnates; see Robert of Torigny, *Chronicle*, 162, 203, 240–41.

¹⁸ See note 4, above.

¹⁹ For the same reason, discussion of anticipatory association in the Byzantine empire has been excluded. Although the German emperors surely knew of such cases, it is uncertain whether the Capetians

MORE FRUITFUL INSIGHTS are gained if the range of comparison is enlarged to include the greater nobility—that is, if Capetian practice is compared not with that of dissimilar monarchies but with the customs of other strata within French society. Anticipatory association in princely and baronial successions has never been systematically studied. Many examples of it have long been known, but those cited in the standard scholarly manuals all date from after 987—beginning, in fact, soon after the association of Robert II with Hugh Capet. These cases have been seen, therefore, as baronial imitations of Capetian royal practice.²⁰ If the single occurrence of the measure among the late Carolingians—the association of Louis V with Lothair in 979—has been considered, the precedent for the nobility has been seen as larger than merely Capetian, and thus the royal character of the practice and its use to buttress disputed hereditary right have seemed to be confirmed.²¹

Different perspectives are supplied, however, by instances of the same practice among the great nobility, some of which predate 989. The earliest example that I have seen comes from the Capetian ancestors: the association of Hugh the Great with Robert I in the latter's county and in the abbacy of Saint-Martin of Tours, which was attested in 913.²² By the 960s other cases of the measure appeared among the dukes of Normandy, the counts of Mâcon, Flanders, and probably Blois, and the viscounts of Dijon. By 989 the duke of Aquitaine also used anticipatory association of the heir.²³ Numerous examples

did. The only reference to such practice by the Byzantines that I have seen in the sources regarding France appears in a letter from Pope Alexander III to Archbishop Henry of Reims and dates from 1171 or 1172; *HF*, 15: 925 (Jaffé, no. 12193).

²⁰ Achille Luchaire, *Manuel des institutions françaises: Période des Capétiens directs* (Paris, 1892), 239–40; and Émile Chénou, *Histoire générale du droit français public et privé des origines à 1815*, 2 (Paris, 1929): 247–49. In contrast to royal practice, anticipatory association among the nobility has been viewed largely as a means of preserving the indivisibility of the great holdings. Although this purpose sometimes lay behind its use, it was neither the sole nor necessarily the principal one; see pages 915–16, below.

²¹ For this association, see Lot, *Les derniers Carolingiens*, 108–09. In this instance, the challenge came from the king's own brother, Charles of Lorraine.

²² Karl Ferdinand Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums (9.–10. Jahrhundert) (i)," *Die Welt als Geschichte*, 18 (1958): 287.

²³ For Normandy (William Longsword with Rollo, 920s), see Dudo of Saint-Quentin, *De moribus et actis primorum Normannie ducum*, ed. Jules Lair, *Mémoires de la Société des Antiquaires de Normandie*, 3d ser., 3, pt. 2 (1865): 173, 181–83; and Ferdinand Lot, *Fidèles ou vassaux?* (Paris, 1904), 183 n. 5. For Mâcon (Leotaldus with Alberic I, 935, and Alberic II with Leotaldus, 960), see Barthélemy Rameau, "Les comtes héréditaires de Mâcon," *Annales de l'Académie de Mâcon*, 3d ser., 6 (1901): 130, 136. For Flanders (Baldwin III with Arnulf I, 958–62), see Léon Vanderkindere, *La formation territoriale des principautés belges*, 2 vols. (2d ed., Brussels, 1902), 2: 292–93. For Blois (Odo I with Theobald le Tricheur, 950s), see Benjamin Guérard, ed., *Cartulaire de l'abbaye de Saint-Père de Chartres*, 2 vols. (Paris, 1840), 1: 54, 2: 351. Interpretation of these charters is uncertain, because additions to the subscriptions to the documents were made after the dates of the acts themselves; see Ferdinand Lot, *Études sur le règne de Hugues Capet et la fin du X^e siècle* (Paris, 1903), 398 n. 2. But in the same note Lot took the Count Odo who subscribed another charter for St.-Père to be Odo I, and that act is from 954; see Guérard, *Cartulaire de l'abbaye de Saint-Père*, 1: 199. That Theobald is called *nobilissimus comes* and Odo only *comes* in one of these charters may imply that Theobald was still alive and that he was the senior count at the time of Odo's subscription (950?); see Guérard, *Cartulaire de l'abbaye de Saint-Père*, 2: 351. For Dijon (Ralph with Robert, 958 or 951), see Lot, *Les derniers Carolingiens*, 329–30, 326 n. 1. For Aquitaine (William V with William IV), see Walther Kienast, *Der Herzogstitel in Frankreich und Deutschland (9. bis 12. Jahrhundert)* (Munich, 1968), 196 nn. 144, 146. Two other apparent cases of such practice must be discounted. Karl Ferdinand Werner has demonstrated that the instance formerly accepted for the county of Troyes in 980–81 did not occur; Werner, "Die Nachkommen Karls des Grossen bis um das Jahr 1000 (1.–8. Generation)," in Wolfgang Braunsfels and Percy Ernst Schramm, eds., *Karl der Grosse: Lebenswerk und Nachleben*, 4 (Düsseldorf, 1967): 462 at 7.4. Two charters from Anjou show the heir to that county as "count" in 976 and probably in the 980s; but, since both are known only from an unreliable textual tradition and one of them is

of it are recorded from the eleventh century, when such cases occurred again in Normandy, Mâcon, Flanders, and (variably) either Blois or another holding of that family and in the comital families of Amiens and Valois, Beaumont-sur-Oise, Meulan, Nevers, and others, as the heirs of those counts were associated in part or all of their prospective inheritances and in the comital title.²⁴ Other such cases can be cited for the twelfth and early thirteenth centuries.²⁵

The chronology of these cases indicates that the great nobles did not simply adopt the practice from a royal model. The pattern was more complicated than that. It cannot be assumed that the tenth-century magnates knew of the Carolingian precedents of more than a century earlier, which for modern schematizations have marked the usage as royal in origin. It is more likely that the tenth-century nobles followed contemporaneous examples familiar to them. One obvious model for the anticipatory associations of the sons of Lothair and Hugh Capet was the case of Otto I of Germany, maternal uncle to both of them, who had his son crowned king in 961 and made co-emperor in 967.²⁶ The lines of probable influence do not, however, point simply to Otto; while Lothair and Hugh certainly knew of the German example,

demonstrably interpolated, both charters must be disregarded unless other evidence appears. See Arthur Bertrand de Broussillon, ed., *Cartulaire de Saint-Aubin d'Angers* (Angers, 1896), nos. 211, 281; and Guillot, *Le comte d'Anjou*, 1: 139 n. 43.

²⁴ For Normandy (Richard III with Richard II, 1025, and Robert Curthose with William the Conqueror, 1065/6–1087), see William of Jumièges, *Gesta Normannorum ducum*, ed. Jean Marx (Rouen and Paris, 1911), 96–97; Marie Fauroux, ed., *Recueil des actes des ducs de Normandie, 911–1066* (Caen, 1961), nos. 34, 36, 55; and David, *Robert Curthose*, 12. For Mâcon (Guy with Otto-William, 997–1003; Otto II with Otto-William, 1005–26; and Geoffrey with Otto II, 1031), see Rameau, “Les comtes héréditaires de Mâcon,” 141–44; Alexandre Bruel, ed., *Recueil des chartes de l'abbaye de Cluny*, 6 vols. (Paris, 1876–1900), 3: nos. 2387, 2406, 2484, 4: no. 2852; and *HF*, 10: 585, 597. For Flanders (Robert II with Robert I, 1086–93), see Charles Verlinden, *Robert I^{er} le Frison, comte de Flandre: Étude d'histoire politique* (Antwerp, 1935), 135–37; and Fernand Vercauteren, ed., *Actes des comtes de Flandre, 1071–1128* (Brussels, 1938), nos. 5, 7–9, 12, etc. For Blois-Champagne (Theobald III and Stephen with Odo II, 1034–37, and Stephen-Henry with Theobald III, 1074–89), see Henri d'Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 7 vols. (Paris, 1859–69), 1: 355–56, 378, 470–73, 396–99; and Prou, *Recueil des actes de Philippe I^{er}*, nos. 67, 78, 86. For Amiens-Valois (Simon with Ralph IV, 1069–74), see *HF*, 11: 433–34. Earlier cases from the same family are probable, but the evidence for them consists of the sons' subscriptions with the comital title to the father's charters; and it is possible that the sons' subscriptions or titles were added after the father's death; for Walter III and Ralph III with Drogo, 1120s(?), see Ferdinand Lot, *Études critiques sur l'abbaye de Saint-Wandrille* (Paris, 1913), no. 8. Also see Prou, *Recueil des actes de Philippe I^{er}*, no. 163. For Beaumont-sur-Oise (Geoffrey with Ivo II, 1039–59), see Joseph Depoin, *Les comtes de Beaumont-sur-Oise et le prieuré de Sainte-Honorine de Conflans* (Pontoise, 1915), 22–24, 48–49, 228. The appearance of another Count Geoffrey of Beaumont in 1023, during the lifetime of Count Ivo I, may indicate an earlier case of anticipatory association in this family; see Fauroux, *Recueil des actes des ducs de Normandie*, no. 25; and Depoin, *Les comtes de Beaumont-sur-Oise*, 20–21. For Meulan (Hugh III with Waleran I, 1056–68), see Jules Joseph Vernier, ed., *Chartes de l'abbaye de Jumièges (v. 825 à 1204)*, 1 (Rouen and Paris, 1916): no. 26; Lot, *Études critiques sur l'abbaye de Saint-Wandrille*, no. 32; and Joseph Depoin, *Cartulaire de l'abbaye de Saint-Martin de Pontoise* (Pontoise, 1895–1909), 311 n. 315. For Nevers (Reginald I with Landri, 1005–28, and Reginald II with William I, 1079), see René de Lespinasse, *Le Nivernais et les comtes de Nevers*, 1 (Paris, 1909): 219; *HF*, 10: 585, 597; and Prou, *Recueil des actes de Philippe I^{er}*, no. 95. Some holders of lesser honores also associated their sons; see, for the viscounty of Chartres in 1036–60, for example, Adolphe de Dion, “Le Puiset aux XI^e et XII^e siècles,” *Mémoires de la Société archéologique d'Eure-et-Loir*, 9 (1889): 8–10, 5–6. The importance of the practice should be judged not simply by the number of cases but also by the length of time such arrangements were in effect in any region; accordingly, in this and subsequent notes the dates shown for anticipatory associations are in each instance those of the duration of the measure—that is, usually from the earliest record of it until the father's death.

²⁵ See notes 31, 40, 45–46, 49–52, 70, below.

²⁶ See note 2, above. For the kinship, see Werner, “Die Nachkommen Karls des Grossen bis um das Jahr 1000,” table, nn. at 6.47, 7.9.

they would also have known that their kinsman, Count Arnulf I of Flanders, had made his son, Baldwin III, count with him in 958. They would have known, too, of the cases from Blois in the 950s and Mâcon in 960.²⁷ The geographical distribution of the examples is significant. In a period when royal activity was increasingly restricted to the region of Paris, these cases were found on all sides of the king's lands: the earliest examples appeared to the northwest, west, south, and southeast of the Ile-de-France; later instances completed the circle. To the received, chronological or "vertical," model of anticipatory association in Capetian practice may be added "horizontal" planes, as similar cases among the nobility are arranged by region, date, and known contacts among the persons to form some image of what any particular generation could have known. From this context, multiple influences—some royal, others princely or baronial—are discernible as background for later practice, particularly after the middle of the eleventh century. The source and nature of the precedents, as contemporaries would have perceived them, thus varied from case to case.²⁸

What is most striking from this evidence is not that certain nobles practiced anticipatory association but that many, or even most, of them did so when their hereditary titles to their honors and lands were not in dispute. From Rollo through William the Conqueror, for example, every duke of Normandy but one either associated his eldest son in the ducal title or formally designated him as heir.²⁹ Whatever may have been the case under the first three dukes, it is not plausible that Richard II, the fourth of his line, or William the Conqueror, the seventh duke and sixth generation of the family, took this step because of weak hereditary right.³⁰ The same may be said of William Talvas,

²⁷ See note 23, above. As for the kinship, Hugh Capet and the wife of Arnulf I were descended from Herbert I of Vermandois, Lothair and Arnulf from Charles the Bald and from Alfred of Wessex; see Werner, "Die Nachkommen Karls des Grossen bis um das Jahr 1000," table, nn. at 6.4, 6.30, etc.; and Erich Brandenburg, *Die Nachkommen Karls des Grossen (I.-XIV. Generation)* (Leipzig, 1935), tables 1, 5.

²⁸ In central France, for example, Aldebert III of La Marche had, by 1086, associated his eldest son in his county; *AVD*, 227. Aldebert III had attended the coronation of Philip I and may have been influenced directly by the royal example; *HF*, 11: 33. But he would also have known that his neighbor, Aldebert II of Périgord, had associated his son in that county by 1080; *AVD*, 203. Aldebert of Périgord had not attended the coronation of Philip I, though doubtless he knew of it. In the viscounty of Limoges, the case of anticipatory association recorded 1111–24 was far removed from any royal prototype; see Maurice Ardat, "Liste chronologique et numismatique des vicomtes de Limoges," *Congrès scientifique de France, 26 session tenue, à Limoges, en septembre 1859*, 2 (Paris, 1860): 297; and *AVD*, 254. Similar variations could be cited from other regions. So complex was the derivation of precedents that for the local populations sometimes, or probably often, the regional nobility furnished the models by which the distant kings' were conceived; see my "L'idée de successions royale et baroniale chez Bernard Itier," *Annales du Midi* (forthcoming).

²⁹ For the anticipatory associations, see notes 23–24, above. For the designations of Richard I by William Longsword and of Richard II by Richard I, see Dudo of St.-Quentin, *De moribus et actis primorum Normanniae ducum*, 220–22, 297; for that of William the Conqueror by Robert I, see William of Jumièges, *Gesta Normannorum ducum*, 111–12.

³⁰ Doubtless William's attitude was influenced by the disorders of his own minority and by the rebellions against him by members of his own family; see David C. Douglas, *William the Conqueror: The Norman Impact upon England* (Berkeley and Los Angeles, 1964), 37–55, 62–66. Also see, for the deathbed speech that Orderic Vitalis attributed to William, Orderic Vitalis, *The Ecclesiastical History of Orderic Vitalis*, ed. and trans. Marjorie Chibnall, 4 (Oxford, 1973): 82–84. The position of Robert Curthose as heir was, however, considerably stronger than William's had been, especially since Robert's grandfather, Baldwin V of Flanders, and his cousin, Philip I of France, would certainly have moved to defend him had his right to the succession been challenged in the event of William's early death.

the twelfth-century count of Ponthieu and Alençon, who, although the sixth generation and seventh member of his line to hold Ponthieu, associated with himself in that county first his eldest son and then his grandson, upon the son's early death.³¹

Reconstruction of the custom as it probably appeared to contemporaries yields similar conclusions. Formal statements as to the nature of anticipatory association are rare and very partial. No general explanation of the usage may exist at all.³² Even statements of the motivation in particular instances are unusual, perhaps in part because of the scarcity of narrative texts. Most cases of the practice are known only incidentally, from the titles used in the sons' subscriptions to charters issued during the fathers' lifetimes. Perhaps the best early source for contemporary ideas on the subject is the history of the dukes of Normandy written at the beginning of the eleventh century by Dudo of Saint-Quentin, chaplain to Duke Richard I. Although Dudo did not formally explain the principles of Norman succession, his views may be inferred from comments throughout the text. Clearly, he assumed that the ducal rule was hereditary: in one passage he explicitly called it a *regnum haereditarium*.³³ Against this background, his portrayal of the actual practices of ducal succession is remarkable. Describing the successions to the first three dukes, he attributed the anticipatory association or designation of their heirs to a variety of motives: because Rollo was old and no longer able to lead his followers into battle, he made William Longsword duke; William Longsword designated and had the barons swear fealty to Richard I because William, out of concern for his son's youth, wanted to assure stability for his domains through a smooth succession; Richard I, in anticipation of his death, designated Richard II because the barons had asked him which of his sons was to succeed.³⁴

It is impossible to assess Dudo's reliability on the events themselves, but as a reporter of eleventh-century attitudes he is authoritative. In this regard, the credibility of his account is confirmed by the chronicle of his near-contemporary, the Burgundian Rodulf Glaber, who recorded the anticipatory associations of the kings of France in almost exactly the same terms as those that Dudo independently used to describe the Norman succession. Glaber attributed the association of Louis V with Lothair to the father's fear that his son might otherwise be denied the throne, that of Robert II with Hugh Capet to the old king's failing health and consequent inability to control a turbulent baronage, and that of Henry I with Robert II to the father's selection of which of his sons would succeed him.³⁵ The account of the association of Robert II

³¹ Clovis Brunel, ed., *Recueil des actes des comtes de Pontieu (1026-1279)* (Paris, 1920), v-vi, nos. 23-26, 48-49, *passim*.

³² I have seen none, and, because of the reasoning outlined below, I doubt that there are any; see pages 923-25 below.

³³ Dudo of St.-Quentin, *De moribus et actis primorum Normanniae ducum*, 220. For Dudo as chaplain, see Fauroux, *Recueil des actes des ducs de Normandie*, no. 13. For his other close ties to the ducal family, see his *De moribus et actis primorum Normanniae ducum*, 18-21, 119-20, 125, 295.

³⁴ Dudo of St.-Quentin, *De moribus et actis primorum Normanniae ducum*, 173, 181-83, 220-23, 297.

³⁵ Maurice Prou, ed., *Raoul Glaber: Les cinq livres de ses histoires (900-1044)* (Paris, 1886), 9, 26, 84.

with Hugh Capet is inaccurate, but for present purposes the value of the record as a whole is what it shows of contemporary beliefs.³⁶ In their statements on anticipatory associations, the writings of Dudo and Rodulf Glaber are mutually corroborative that the practice did not have the single and constant meaning that modern scholarship has ascribed to it; rather, it was subject to quite varied interpretation at the time. The evidence of their accounts is the weightier, moreover, since the two men wrote in different decades, about different events, and in localities far removed from each other, yet for audiences that both presumed would share their views.

The lessons of these texts may be extended to other regions. The surviving narrative texts contain only fragmentary data for this topic, but a considerable body of relevant information can be pieced together from scattered references in those works and from the extant charters. From this record it appears that a contemporary observer who knew of anticipatory association chiefly from the perspective of a close follower of the king would have had little reason to connect it with a disputed hereditary title. Examination of the documents from the reign of Philip I (1060–1108) allows partial reconstruction of the composition of the king's entourage and of his contacts with the nobility from outside his usual circles. Of those nobles who used the practice, only the usurper Robert I of Flanders, who was almost never at Philip's court, can be said to have done so because of a contested title.³⁷ For the others, the sources either imply other reasons for the practice or record none at all. Of the two cases in which Philip I is known to have taken part in the ceremony of association, neither seems to have involved a disputed succession, and other families who employed the measure and whose members figured occasionally in the royal circle also appear to have been secure in their hereditary rule.³⁸

ANTICIPATORY ASSOCIATION OF THE HEIR in France was thus not a distinctively royal usage. Nothing tied it peculiarly either to the Capetian dynasty or to the crown. It was a feature of numerous successions, and thus successions provide the framework within which it may be understood. In noble society the anticipatory association of the heir might have any of several possible functions, only one of which was the reinforcement of a weak hereditary title. It was sometimes a means of transmitting the father's entire inheritance to his eldest son to the exclusion of the cadets. In other cases,

³⁶ Note that another Burgundian writer of ca. 1050 gave substantially the same version of Robert II's association with Hugh Capet; see the *Vita Garnerii*, in *HF*, 10: 382.

³⁷ For this association, see the references in note 24. above; for the lasting imputation of illegitimacy in this case, see Galbert of Bruges, *Histoire du meurtre de Charles le Bon, comte de Flandre* (1127–1128), ed. Henri Pirenne (Paris, 1891), 110–15.

³⁸ For the instances of Philip's participation, see the references for the associations of Robert Curthose of Normandy in 1065–66 in note 24. above, and of Fulk V of Anjou in 1106 in note 45. below. For the presence in Philip's entourage of the members of other families who practiced anticipatory association—notably Theobald III of Blois and his son, Stephen-Henry; Ralph IV of Valois and his sons; and William I of Nevers and his son, Reginald—see Prou, *Recueil des actes de Philippe I^{er}*, nos. 2, 22–23, 39, 66–67, 78, 86, 95.

when the family had several holdings, it was used in conjunction with a partition of the estate or the assignment of apanages as one element in a more complicated successional plan. Occasionally, it provided a coadjutor to an ailing, elderly father.³⁹ Yet again, it could mark a recognition or an endowment of a prospective heir who had come of age; in cases of this sort, the association was sometimes linked to the marriage of the heir and the assignment of a dower to his wife.⁴⁰

This description sketches the possible arrangements only in broad outline. Variations were numerous. In 995 or 996, for example, Duke Richard I of Normandy appointed his eldest son to succeed him and specified which lands should be given to the cadets. Richard II, by contrast, formally associated his eldest son in the duchy and invested his second son with a county but left it to his successors to make provision for the younger cadets.⁴¹ Odo II of Blois and Champagne, who in the 1030s provided for a future division of his holdings between his two sons, made both of them counts during his own lifetime. His son, Theobald III, when effecting a similar partition among his own sons, made only the eldest of them count before his own death.⁴² Some cases of anticipatory association provided the heir with support from a portion of the family's property until he came into his full inheritance.⁴³ In others, when the father had two or more counties to leave to his sons, the eldest might be associated in his entire share of the succession.⁴⁴

In exceptional cases, the anticipatory association was enacted by initiative other than the father's. Thus, in 1103 the intention of the old count, Fulk IV

³⁹ For the transfer of the entire inheritance to the eldest son, see, for example, the associations of Reginald I of Nevers and Geoffrey of Mâcon; note 24, above. Also see notes 49–50, below. Capetian parallels are the successions to Henry I and Louis VI; see notes 12, 14, above. For partitions of estates, see the successions to Richard II of Normandy, Odo II and Theobald III of Blois and Champagne, and William Talvas of Ponthieu; notes 41–42, below, and note 31, above. A Capetian parallel is the succession to Robert II; see pages 908–09, above, and Jan Dhondt, "Note sur les deux premiers ducs capétiens de Bourgogne," *Annales de Bourgogne*, 13 (1941): 30–38. For William Longsword as coadjutor with Rollo in Normandy, see Dudo of St.-Quentin, *De moribus et actis primorum Normanniae ducum*, 173, 181–83. This same motive probably figured in the associations of Baldwin III with Arnulf I of Flanders and of Fulk V with Fulk Réchin of Anjou; see note 23, above, and page 917, below. A Capetian parallel is the association of Philip Augustus with Louis VII; see note 15, above.

⁴⁰ This same motive was the reason for the association of John, son of Robert III of Alençon, in 1205; see Henri-François Delaborde et al., eds., *Recueil des actes de Philippe Auguste, roi de France*, 3 vols. (Paris, 1916–), 2: no. 905. It was one of the considerations in that of Geoffrey Plantagenet with Fulk V of Anjou in 1127–28; see page 917, below. The association of Robert II with Robert I of Dreux in 1184 coincided with or slightly preceded the second marriage of Robert II; André Duchesne, *Histoire généalogique de la maison royale de Dreux* (Paris, 1631), 45, 238, 248. Either an association or measures similar to one were effected in 1189 at the time of the marriage of the heir to the county of Meulan; Étienne Deville, ed., *Cartulaire de l'église de la Sainte-Trinité de Beaumont-le-Roger* (Paris, 1912), nos. 255, 261. But see Depoin, *Cartulaire de l'abbaye de Saint-Martin de Pontoise*, 323–24.

⁴¹ For Richard I, see Dudo of St.-Quentin, *De moribus et actis primorum Normanniae ducum*, 297. For Richard II, see William of Jumièges, *Gesta Normannorum ducum*, 96–97. For corroboration of William of Jumièges's account, see Fauroux, *Recueil des actes des ducs de Normandie*, no. 55. It was, in fact, the young William the Conqueror who endowed these cadets; William of Jumièges, *Gesta Normannorum ducum*, 119.

⁴² Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 1: 378, 355, 472–73, 396–99, 425–26.

⁴³ For Theobald III's possession of the county of St.-Florentin during his father's lifetime, see Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 1: 378; for Robert II of Flanders, see Verlinden, *Robert I^{er} le Frison*, 136–37; and, for Robert II as "lord of Dreux" but without his parents' other holdings during his father's lifetime, see Duchesne, *Histoire généalogique de la maison royale de Dreux*, 238, 248.

⁴⁴ Thus, for Leotaldus, Guy, and Otto II of Mâcon, see Rameau, "Les comtes héréditaires de Mâcon." 130–31, 141–42; and for Guy II and John of Ponthieu, see Brunel, *Recueil des actes des comtes de Pontieu*, v–vi.

Réchin of Anjou, to disinherit Geoffrey Martel, his son of his second marriage, in favor of Fulk V, his son by his third wife, Bertrada of Montfort, drove Geoffrey to rebellion. Geoffrey prevailed by force and compelled his father to associate him as count as the price of reconciliation. Three years later, Geoffrey was killed in battle in Normandy. Upon learning of his death, King Philip I, step-father of Fulk V, invested the latter with the county of Anjou, acting apparently on his own authority as overlord and without known consultation with the youth's father.⁴⁵ Occasionally, the same measure was used by a childless count, either a bishop who had inherited a county or a layman, to designate which of his collateral relatives should succeed him.⁴⁶ In some instances, the young count was largely independent of his father; in others, he was clearly subject to paternal control.⁴⁷

From the late eleventh century onward, variations linked to pilgrimage or crusader activity appeared. Thus, Robert II of Flanders, who earlier had been styled count in a few charters of his father and others (perhaps because he had been proclaimed heir), was officially invested with the title in 1086, immediately prior to his father's departure for the East; and in that capacity he governed the county as regent during his father's absence. After Robert I's return the son retained the comital style, although he continued to administer only a portion of the county.⁴⁸ By contrast, the association of Geoffrey Plantagenet with his father Fulk V as count of Anjou in 1127 or 1128, at the time of Geoffrey's betrothal and marriage to the Empress Matilda, became a permanent transfer of the county when within two years Fulk departed Europe to marry the heiress to the kingdom of Jerusalem.⁴⁹ A generation later, Fulk's son-in-law, Thierry of Alsace, count of Flanders, made comparable, if broader, arrangements for his succession prior to his own trips to the Holy Land. In 1157, on the eve of his third such trip, Thierry provided ecclesiastical

⁴⁵ For the associations themselves, see Joseph Chartrou, *L'Anjou de 1109 à 1151: Foulque de Jérusalem et Geoffroi Plantagenêt* (Paris, 1928), 1–4. For problems in the interpretation of both of these associations, see Guillot, *Le comte d'Anjou*, 1: 117 n. 528, 123 n. 563. But see notes 55, 57, below. These events may be of particular importance for an explanation of Philip I's decision not to have the future Louis VI consecrated during his own lifetime and to grant Louis only the title *rex designatus*; see note 13, above. Philip I's direct knowledge of the sometimes adverse consequences of anticipatory association included not only a familiarity with the problems of Fulk Réchin but also the lessons of his own involvement in the settlement of one revolt by Robert Curthose against William the Conqueror; see Douglas, *William the Conqueror*, 236–39, 239 n. 4.

⁴⁶ For the association in the 1030s of Theobald with his uncle, Hugh, bishop of Auxerre and count of Chalon, see Bruel, *Recueil des chartes de l'abbaye de Cluny*, 4: nos. 2846, 2848, 2905, etc. The case of Ivo, count of Soissons and lord of Nesle, and his nephew, Cono, is more complicated. In 1157 Ivo declared Cono his heir if he himself died without issue; in 1172 Cono was called Ivo's successor; from 1176 until Ivo's death in 1178, both men appear with the titles "count of Soissons" and "lord of Nesle"; see William Mendel Newman, *Les seigneurs de Nesle en Picardie (XII^e-XIII^e siècle): Leurs chartes et leur histoire*, 2 vols. (Paris and Philadelphia, 1971), 1: 34–35; 2: nos. 29–30, 58, 73–78.

⁴⁷ Among sons or grandsons who were largely independent were Leotaldus of Mâcon, Stephen-Henry of Blois, and Guy II and John of Ponthieu. See Bruel, *Recueil des chartes de l'abbaye de Cluny*, 1: no. 432; Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 1: 396–400; and Brunel, *Recueil des actes des comtes de Ponthieu*, nos. 23–26, 48–49, *passim*. For sons largely dependent, note the heirs of Odo II of Blois and note Robert Curthose of Normandy and Robert II of Flanders. Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 1: 317–20, 333, 343, 378, 472–73; David, *Robert Curthose*, 13, 19, 29, *passim*; and Verlinden, *Robert I^{er} le Frison*, 135–37.

⁴⁸ Verlinden, *Robert I^{er} le Frison*, 135–37, 21, 21 nn. 2–3.

⁴⁹ Chartrou, *L'Anjou de 1109 à 1151*, 21–24; and C. Warren Hollister and Thomas K. Keefe, "The Making of the Angevin Empire," *Journal of British Studies*, 12 (1973): 15.

placement for two of his cadets and established Philip, his eldest living son, as "heir and count of all Flanders" under the protection of his kinsman, Henry II of England. Seven years later, after the estates of his other children had been assured through marriage, Thierry departed again, to settle permanently in the East, leaving Philip with the full comital title as his successor in Flanders.⁵⁰

Distinctive cases are recorded for the French possessions of the kings of England. In 1120 Henry I made his son, William Adelin, duke-designate of Normandy, not only to guarantee his eventual succession despite the challenge from William Clito, but also to perform homage for the duchy to Louis VI, which Henry himself refused to do.⁵¹ In 1169 Henry II associated his eldest living son, the young Henry, as duke of Normandy and count of Anjou and made Richard, the second son, count of Poitiers with Eleanor of Aquitaine as part of a comprehensive successional plan.⁵²

A similar variety of arrangements appeared in cases involving the sons of widows who held lordships in their own right. In such situations, the son frequently assumed the title of his mother's honor during her lifetime. It is unclear whether these cases are strictly comparable to those of anticipatory association with a father, because—although all such measures of coseigniorship imply the son's right of inheritance—some cases in which the son took his mother's title indicate that he was the chief lord and active administrator of the holding, as if she were ineligible to exercise lordship. Thus, in the eleventh century the countess of Vendôme was little more than an arbiter in the fate of her county, which was held and governed in turn by her father, her half-brother, and her sons rather than by herself. Again, in the first decades of the twelfth century, the widowed countess of Vermandois saw her county placed under royal wardship during the minority of her son, who was recognized first as count-designate and then as count during his mother's lifetime. In the 1090s, there was even shared rule at Bourges between the husband of the viscountess and their son-in-law, with both men simultaneously entitled lord of Bourges.⁵³ But variations and contrasts are found. At the end of the twelfth

⁵⁰ Adriaan E. Verhulst, "Note sur une chartre de Thierry d'Alsace, comte de Flandre, pour l'abbaye de Fontevault (21 avril 1157)," in *Études de civilisation médiévale (IX^e-XII^e siècles): Mélanges offerts à Edmond-René Labande* (Poitiers, 1974), 711-19; and Robert of Torigny, *Chronicle*, 193, 220. For Thierry's children, see Vanderkindere, *La formation territoriale des principautés belges*, 1: 301-15.

⁵¹ Hollister, "Normandy, France, and the Anglo-Norman *Regnum*," 224-27. There was a partial precedent for Henry's action in the policy of William the Conqueror with regard to Robert Curthose and the county of Maine. Robert twice did homage for Maine to the counts of Anjou. In 1063 or 1064 he did so as fiancé of Margaret, minor heiress to the county. But he performed the second homage, in 1081, as surrogate for his father after William's second conquest of Maine; Margaret had died some years earlier, before celebration of the marriage, and the best title for the claim to Maine was a dubious agreement by which Herbert II of Maine was alleged to have made William his heir. See David, *Robert Curthose*, 9-11; Robert Latouche, *Histoire du comté du Maine pendant le X^e et le XI^e siècle* (Paris, 1910), 32-39; and Guillot, *Le comte d'Anjou*, 1: 105-06, 105 n. 470, 119-20.

⁵² Warren, *Henry II*, 108-11.

⁵³ For the countess of Vendôme, see Charles Métails, ed., *Cartulaire de l'abbaye cardinale de la Trinité de Vendôme*, 1 (Paris, 1893): no. 6; and Guillot, *Le comte d'Anjou*, 1: 27 n. 139, 38 n. 184. For the countess of Vermandois, see Luchaire, *Louis VI le Gros*, nos. 35, 81, 123, 134, 166, 299. The mother-countess Adela styled herself *Viromandensium comitissa* and her two elder sons *futuri comites* in a charter of 1103; Bibliothèque Nationale, Paris, MSS Coll. Moreau, 41, f. 99. In 1109 the eldest son, Ralph, was called *Vermandorum futurus*

and beginning of the thirteenth centuries, almost simultaneously, the son of the widowed lady of Venisy (Aube) held and did homage for that castle, while the son of the widowed countess or lady of Braine (Aisne) used only the title he had inherited from his father and avoided mention of his mother's lordship in his titulary for as long as she lived.⁵⁴ Clarification of these inconsistencies must await systematic study of the chronological and regional differences in the property rights of women.

The diversity of these cases warns against hasty adoption of a narrow definition of anticipatory association. Although, from a juridical or strict procedural criterion, the essential element of the measure was the formal association of the heir in the office of his father or other relative, the substance—both in intention and in practical effect—varied from case to case. Numerous contemporaries, moreover, appear to have been unimpressed by precisely the aspect that usually seems most important to modern scholars, for the son who had been associated with his father was not always given the father's title in the documents. In some cases, the son appears with the title; in others, he is only styled son of the titled parent.⁵⁵ These inconsistencies in diplomatics are attributable in part to the usages of different scribes, for in the eleventh and twelfth centuries many royal and baronial charters were drawn not by the donor's chancery but by clerks of the recipient ecclesiastical establishments.⁵⁶ The usage reflects also a lack of stress on the son's title in the

comes in an act of Louis VI, and the subscriptions gave him the same title; Edmond Martène and Ursin Durand, eds., *Veterum scriptorum et monumentorum amplissima collectio*, 1 (Paris, 1724): 624–25. In 1110 Ralph, independently of his mother, issued a charter in which he was styled “per gratiam Dei designatus Viromandensium comes”; William Mendel Newman, ed., *Charters of St.-Fursy of Peronne* (Cambridge, Mass., 1977), no. 7. In 1114 Adela herself styled Ralph (but neither of her other sons) *comes*; Victor de Beauvillé, *Histoire de la ville de Montdidier*, 1 (2d ed., Paris, 1875): 485 no. 2. Adela remained active until the end of her life; her charters style her countess but sometimes do not use the comital title for her son; see, for example, Joseph Depoin, ed., *Recueil de chartes et documents de Saint-Martin-des-Champs, monastère parisien*, 1 (Paris, 1912): no. 161. For the viscountess of Bourges, see Philippe Labbé, *Histoire du Berry abrégée dans l'éloge panegyrique de la ville de Bourges* (Paris, 1647), 192–93. The charter is explicit in noting that both mother and daughter were living.

⁵⁴ Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 3: app. 22, no. 285: “Dominus Erardus de Venisi tenet castrum de Venisi a domino Campanie. . . .” This arrangement is the more remarkable since Erardus's mother, Alice of Venisy, had remarried, and her second husband sometimes styled himself “lord of Venisy”; Maximilien Quantin, ed., *Recueil de pièces pour faire suite au Cartulaire général de l'Yonne* (Auxerre, 1873), no. 103. Also see *ibid.*, nos. 24, 245; and Newman, *Les seigneurs de Nesle en Picardie*, 2: 340 n. 5. For the property, see Charles Lalore, *Documents pour servir à la généalogie des anciens seigneurs de Trainel* (Troyes, 1872), nos. 72–73, 124–25. For Agnes, countess of Dreux and Braine, and her son, Robert II, see Duchesne, *Histoire généalogique de la maison royale de Dreux*, 250–51. In anticipation, however, the charters of others sometimes referred to the son as “count of Braine”; *ibid.*, 239.

⁵⁵ For references to Theobald III and Stephen-Henry of Blois and Champagne, for example, see Prou, *Recueil des actes de Philippe I^{er}*, nos. 67, 78, 82. Walter, son of Ralph IV of Valois, was styled variably either count or Ralph's son; *ibid.*, nos. 2, 22. It is unclear here, however, whether the comital title implies an association of Walter with Ralph or refers to Walter's maternal inheritance of Bar-sur-Aube. One charter refers to William VII, son of Robert II of Auvergne, as *comes* in the body of the act but records his subscription as “S. Wilelmi, filii ejus”; *ibid.*, no. 135. For William as count in another charter from his father's lifetime, see *ATD*, 134. Similarly, a charter of Ralph IV of Valois from 1069 calls his son, Simon, *comes* in the formula of the date but *Comitis filius* in the subscription; *HF*, 11: 433. Such usage long remained mixed in some regions. A charter of Delphinus, count of Auvergne, and his son William from 1199 called William only *filius meus* in the announcement of the act, although William's seal on the document styled him *comes*; *LTC*, 1: no. 501.

⁵⁶ For the best demonstration of this point, see Françoise Gasparri, *L'écriture des actes de Louis VI, Louis VII, et Philippe Auguste* (Paris, 1973), 17–26, *passim*. Also see Newman, *Les seigneurs de Nesle en Picardie*, 2: 11–14.

eyes of contemporaries. Given their premise of hereditary succession, the word "son" often included the connotation "heir," and the authors of these documents did not always consider more specific designation of status necessary.

A hasty reading of the sources might suggest uncertainty as to the son's title, but that inference is unjustified. To take the clearest example, Philip, eldest son of Louis VI, was proclaimed *rex designatus* in 1120, and he was mentioned with that title in numerous royal charters issued between that date and his consecration in 1129; but other royal charters from the same years style him only *filius noster* or *filius regis*. Conversely, in popular usage, though seldom in the formal diplomatics, a son who had not been associated with his father might be called by the latter's title in expectation of his eventual succession.⁵⁷ Finally, a number of other measures that resemble anticipatory association must be viewed in conjunction with it. Thus, in both the nobility and the aristocracy of simple lords, the father might arrange for his son or sons to do homage in advance to his lord for the inheritance, or he might in his own lifetime effect a legal transfer of his holdings to the son, usually the eldest, whom he had chosen as heir.⁵⁸ Other variations could be cited.

Many problems are obscure. It is apparent that anticipatory association was not uniform in character. On the contrary, the variations indicate a diversity, not only of intent, but of probable form and of the relative importance attributed to it from case to case. The ideas behind those instances of the practice that are least emphasized in the sources are barely distinguishable from the premises of the countless charters from nobles and petty lords that so carefully recorded the consent of the heirs, especially that of the eldest son, to alienations from the family property.⁵⁹ Why some families practiced anticipatory association of the heir and others did not is also unclear. Nor is there a sure explanation for the somewhat sporadic character of the custom: that, for example, a given family employed it in one generation but not in the next. In general, the outlines of successions that included the measure appear

⁵⁷ For Philip, see Luchaire, *Louis VI le Gros*, nos. 302, 310, 317, 321, 363, 424; but also see *ibid.*, nos. 365, 408, 419. For similar variations of title for Louis VI as *rex designatus*, see *ibid.*, 291–93, nos. 22, 24, etc. Louis VI is never called "king" in the extant acts of Philip I; Prou, *Recueil des actes de Philippe I^{er}*, cviii–cix. For contemporaneous variations in Anjou, see Guillot, *Le comte d'Anjou*, 1: 117–18 n. 528. For a text in which, after the record of a donation by the son-count to which the father had consented, the latter, who is called count in the body of the act, appears in the subscriptions only as *pater ejus*, see *ibid.*, 2: no. 439. For popular usage, see Lewis, "L'idée de successions."

⁵⁸ Georges Duby, *La société aux XI^e et XII^e siècles dans la région mâconnaise* (Paris, 1953), 173, 279–280. For a case of similar homage from Flanders, see *HF*, 16: 63–64.

⁵⁹ In this regard, there is some confusion in the vocabulary of modern scholarship. Depoin has described Robert II of Meulan as "dès 1157, associé aux actes de son père"; *Cartulaire de l'abbaye de Saint-Martin de Pontoise*, 323. And Lot has written of Richard of Vernon as "associé à son père Guillaume dès 1165"; *Études critiques sur l'abbaye de Saint-Wandrille*, 178 n. 4. But in neither case is there evidence of full anticipatory association. The distinction between anticipatory association and the shared property rights that required the consent of the kinsmen to alienations is often vague; for William and Richard of Vernon, for example, see Léopold Delisle, ed., *Recueil des actes de Henri II, roi d'Angleterre, concernant les provinces françaises, Introduction* (Paris, 1909), 437 n. 6; and, for Richard and his son, see *LTC*, 1: no. 441. An undated charter (ca. 1184?) of Robert I of Dreux to Notre-Dame of Estrée succinctly illustrates the problem: "Ego R. Comes Drocensis [...] Et R. filius meus Comes per Dei gratiam post me futurus et Guillelmus [sic] filius meus et petrus filius meus. Notum fieri volumus . . ."; Archives de l'Eure. H 319, ff. 59r–60r. Also see note 55, above.

to have been the same as in those which did not: the exclusion of cadets from the inheritance, successional partitions, and the granting of apanages are found in both categories. In rare instances, partial explanations for such differences are possible, but more often the extant sources are insufficient for such hypotheses.

ANTICIPATORY ASSOCIATION OF THE HEIR, as a successional phenomenon, was linked to, or was part of, the familial structure of the noble class, since, once the heritability of offices and of the lands attached to them was established, the family became the cadre for the exercise and transmission of the related rights of lordship and property. Within this setting, anticipatory association was one of a number of organizing practices found variably but often in the dynastic arrangement of the noble family that emerged in France in the tenth and early eleventh centuries. As, with the fragmentation of royal and even princely power, the higher nobility established themselves in permanent control over fixed territories, and their rule became hereditary—that is, a patrimony—their families assumed the vertically linear structure of dynasties, in contrast to the earlier clan structure. The families, in effect, organized themselves around their patrimony and its devolution. The sons, or one of them, inherited; the holding then became indivisible, and primogeniture was introduced to preserve it. Cadets might receive provision through the inheritance of lesser holdings, marriage to heiresses, or ecclesiastical placement—or they might be disinherited—while the eldest normally received the undivided patrimony.⁶⁰

The parallel between the Capetians and the greater nobility in this regard is clear. For the Capetians the crown was the family's patrimonial *honor* in direct analogy to the principal *honor* of a noble family. The comparison is not absolute; obviously, some aspects of the royalty precluded a simple equation of it to a barony. No recorded spokesman of the time—that is, no educated cleric or monk—would have regarded the religious office of the king as wholly the private possession of anyone, and the lay magnates doubtless recognized differences between kingship and their own titles. But the election and consecration that set the king apart from other magnates were constitutive acts pertaining to his office. In terms of familial structures, they are nonessential in that through the channels of direct personal inheritance the son possessed the right to his father's office and lands that led to his anointment as king.⁶¹ The

⁶⁰ For these patterns of inheritance, chiefly see Georges Duby, "Structures familiales aristocratiques en France du XI^e siècle en rapport avec les structures de l'État," in Tadeusz Manteuffel and Aleksander Gieysztor, comps., *L'Europe aux IX^e–XI^e siècles: Aux origines des États nationaux*, Actes du colloque international sur les origines des États européens aux IX^e–XI^e siècles, tenu à Varsovie et Poznań du 7 au 13 septembre 1965 (Warsaw, 1968), 57–62. For regional and other variations, see my "The Capetian Apanages and the Nature of the French Kingdom," *Journal of Medieval History*, 2 (1976): 129 n. 14.

⁶¹ Some studies have treated election- and consecration-right as contradictory to a hereditary basis for royal rule. This was not necessarily the case. Recent research suggests that the election of the king of France was less a procedure of selection of the next monarch than it was a consent or acclamation that signified acceptance of him and that obligated the electors to respect his rights and jurisdiction. See,

crown did not alter the basic patterns within which the lay titles and fortunes devolved. The royalty occupied the same place for the Capetians, considered as a family and viewed through the succession, that the duchy of Normandy did for the descendants of Rollo and that various counties did for the families of their counts.

As a successional practice that promoted the dynastic ordering of the family, anticipatory association of the heir is easily understood as one feature of this system. In individual cases its function is sometimes unclear because the uses varied. The measure could help establish the precedents by which the possession became hereditary, as in the association of Robert II with Hugh Capet and in some of the earlier cases among the nobility. In different circumstances the same measure was not the cause but the result of the hereditary character of the holding, as in the association of Philip Augustus with Louis VII and in most examples of the practice among the French nobility of the twelfth century. In short, although the measure, when it was used, was part of the hereditary transmission of the office and property, its occurrence alone is evidence only of the wishes or expectations of the principals. By itself anticipatory association does not indicate whether hereditary succession was an object of dispute or a shared premise in the particular context.

Early Capetian successional usage has thus been judged by an index of variable meaning. Comparison between the French and the German or English monarchies placed in high relief and seemed to confirm as the whole what in reality was only one of the possible meanings of the measure. The fallacy of that approach is rooted in its limited selection of data. By treating the anticipatory association of the heir almost solely as an aspect of constitutional history, narrowly construed, it pulled out of societal context, and applied a static interpretation to, a practice that was part of broader social and political history and accordingly was more supple. The essential elements of its usage are not the shared traits of monarchies considered very nearly in the abstract, but rather the interrelated structures of kinship, property-holding, and rule within the given society. With respect to Capetian succession, the most relevant material comes from the common practices of the other great titled and landed families that surrounded and informed the kings.

especially, Walther Kienast, "Die französischen Stämme bei der Königswahl," *Historische Zeitschrift*, 206 (1968): 1–21; and John M. Wallace-Hadrill, *Early Germanic Kingship in England and on the Continent*, Ford Lectures for 1970 (Oxford, 1971), 133–35. Abbo of Fleury's discussion of royal election in his chapter "De fidelitate regis," in which he argued the magnates' duty to aid the king, supports this thesis; see Abbo of Fleury, *Collectio canonum*, in Jacques-Paul Migne, *Patrologiae cursus completus . . . series latina*, 139 (Paris, 1880): 478. The protocol from the election of Philip I reflects the same idea; *HF*, 11: 32–33. The documents on the associations of the sons of Robert II probably can be read in the same way; see the sources of note 11, above. Schramm's concepts of "freier" and "gebundenere Auswahlen" describe other aspects of the proceedings but do not give full value to this one; *König von Frankreich*, 1: esp. 102. The significance of royal unction is also complex. By ecclesiastical theory, it was the anointment that made the king, and, thus, a man of nonroyal blood could be raised to kingship by the same means. But strict construction of that theory ignores the fact that the French queens were also consecrated, to make them fertile and mothers of offspring worthy of the throne; E. H. Kantorowicz, "The Carolingian King in the Bible of San Paolo fuori le mura" (1955), reprinted in Michael Cherniavsky and Ralph E. Giesey, eds., *Selected Studies by Ernst H. Kantorowicz* (Locust Valley, N.Y., 1965), 82–94; and Wallace-Hadrill, *Early Germanic Kingship*, 132–34. The effect was to make only kings' sons the progeny of unions that had been specially blessed to produce future kings. From this perspective, the royal consecrations strengthened the claims of hereditary monarchy.

THE FIRST LESSON from a comparison of royal with baronial practice, thus, is that the supposed link between anticipatory association and weak hereditary titles is no longer apparent if for focus on the crown one substitutes attention to the common structures of the ruling class within the society. The second lesson, no less important, is that the customs of the French nobility do not allow the fixed, even rigid, institutional character that study of kings alone has suggested for the measure. In restricting its examination to royal practice, the constitutional focus has viewed the eight Capetian cases of anticipatory association chiefly in relation to each other. One effect of this approach has been to draw links between these cases, forming them into an identifiable system from which the strength of Capetian throne-right relative to resistance to the hereditary principle was deduced. The diversity in circumstances and apparent intent of the same measure as used by the nobility casts the bases for this schematization into doubt.

These insights may help clarify why, from the reign of Philip Augustus, the Capetians no longer practiced anticipatory association. Historians have seen Philip's departure from the successional policy of his predecessors as a symbolic turning-point in the establishment of hereditary Capetian rule, but speculation as to the immediate reasons for the change has been vain because of the lack of comment on the subject in the extant sources.⁶² The strongest safe judgment was simply that from that time the Capetians did not consider it necessary to associate their heirs. That opinion assumes, however, that it was necessity that had caused earlier Capetians to practice anticipatory association, and that premise is quite uncertain. For, if the implications of patrimonial succession are accepted, it need not follow either that a disputed hereditary title had required the Capetians to employ anticipatory association to buttress their position or that discontinuance of the custom by Philip Augustus signals a sudden change in their status.⁶³

Since the sources do not explain what Philip's action—or, rather, his inaction—signified, perhaps concern with the question is itself misplaced. Perhaps his decision not to associate his heir had no special significance at all. Although modern scholarship has treated Philip's succession as a constitutional landmark, contemporary writers—not only those who were close to

⁶² There is no foundation in the sources for the suggestion by Lot and Fawtier that the Carolingian descent of Louis VIII through both paternal and maternal lines may explain why Philip Augustus did not associate him on the throne; *Institutions royales*, 16. In fact, although the sources record Carolingian descent for both Philip and his first wife, Isabella of Hainaut, almost no text that mentions the one also notes the latter; see the sources of Werner, "Die Legitimität der Kapetinger," 203–05, 220–25. The knowledge or belief that Philip Augustus and Louis were descended from Charlemagne is documented from the late 1190s, but this descent was not presented as a basis for their rule; see Lewis, "Dynastic Structures and Capetian Throne-Right," 240–47.

⁶³ Petit-Dutaillis has asserted that it was because of the "growth of the royal authority" during the reign of Philip Augustus that Philip "had been able to dispense with" the practice of anticipatory association; *Feudal Monarchy in France and England*, 311. But Philip Augustus appears to have been confident of his son's rights from the time of Louis' birth, long before the great increase in royal power during the second half of the reign; Lewis, "Royal Succession in Capetian France," chap. 3; "Royal Succession in the King's Business: The Evidence of the Chancery Records, 1180–1206." If a symbolic date were to be set for the discontinuance of anticipatory association by the Capetians, the best choice would not be 1223, the year of Philip's death, but rather 1190, the date he departed for a hazardous crusade leaving as heir an unconsecrated child not yet three full years of age.

Capetian circles, but others who were far removed from them—do not appear to have noticed anything unusual about it. Their silence on the point does not mean that they were unaware either that Louis VIII, Philip's successor, had not been associated with his father on the throne or that earlier Capetians had employed such association, for most of the writers knew both. Rather, the occurrence of the same omission in so many texts implies that the chroniclers did not consider this circumstance important.⁶⁴ The only writer who described the accession of Louis VIII as unusual was Vincent of Beauvais, who in the early 1240s traced Carolingian descent for Louis through his mother and claimed that in him the kingdom of the Franks had been "returned to the family of Charlemagne."⁶⁵ Vincent did not, however, link this descent to the fact that Louis had not been associated as king before Philip's death. Indeed, Vincent paid scant attention to anticipatory association, not only with regard to Louis VIII but, more notably, for the early Capetians as well. In his *Speculum historiale*, Vincent recorded, under the appropriate years, the succession of Capetian kings at their fathers' deaths, but he noted or alluded to only three cases of anticipatory association: that of Philip Augustus, together with the paralysis and then the death of Louis VII, in a single chapter on the change of reigns in 1179–80; that of Louis VII in the context of the visit to France by Pope Innocent II, who performed the ceremony; and that of Philip, eldest son of Louis VI, when explaining that the coronation of Louis VII followed the death of his brother, Philip, who "had recently been anointed king."⁶⁶ He did not mention the other cases, although all of them had been recounted in numerous earlier texts.⁶⁷ It would thus appear that to Vincent of Beauvais the whole practice of anticipatory association was of negligible importance.

This attitude accords well with the lessons from baronial usage. If the latter are considered together with the history of Capetian practice, it is apparent that Philip Augustus did not abandon a system, for there was no system to abandon—only a number of instances in which various arrangements of coseigniorship had been used in differing circumstances to achieve differing, though related, ends. By viewing anticipatory association almost solely as a

⁶⁴ For writers close to the court, see William the Breton, *Philippidos libri XII*, in Delaborde, *Oeuvres de Rigord et de Guillaume le Breton*, 2: 348; and the *Chronicon Turonense magnum*, in *HF*, 18: 303–04. For writers from other regions, see, for example, *HF*, 18: 345. Bernard Itier of Limoges did not even record the accession of Louis VIII in any of his extant notes; although he included Philip's name in several lists of the deaths that had occurred during 1223, he did not mention Louis as king until his record of the events of 1224; see Henri Duplès-Agier, ed., *Chroniques de Saint-Martial de Limoges publiées d'après les manuscrits originaux* (Paris, 1874), 114–18. For English writers, see, for example, *HF*, 18: 117–18, 209. For the views of lay magnates, the only direct evidence I have found is that implicit in a charter from 1224 of Philip Hurepel, half-brother of Louis VIII, which speaks of the latter as "post patrem nostrum jam confirmatus in regno"; Léopold Delisle, "Recherches sur les comtes de Dammartin au XIII^e siècle," *Mémoires de la Société impériale des Antiquaires de France*, 4th ser., 1 (1869): 241.

⁶⁵ Vincent of Beauvais, *Speculum historiale*, 30.125–26, in *Bibliotheca mundi seu Speculum maioris praesulis Bellovacensis* . . . , 4 (Douai, 1624): 1275–76. On Vincent and his source for this idea, see Werner, "Die Legitimität der Kapetinger," 203–05, 219–20.

⁶⁶ Vincent of Beauvais, *Speculum historiale*, 29.22, 27.13, in *Bibliotheca mundi*, 1193, 1099.

⁶⁷ For Vincent's other notes on the early Capetian successions, see *Speculum historiale*, 24.93, 24.107, 25.16, 25.35, in *ibid.*, 995, 1001, 1007, 1014.

Capetian practice, many scholars have taken a series of cases that occurred in a single family over the course of two hundred years and have posited an institution of set character, when in fact nothing firm enough to be called an institution existed. Broader examination reveals what is better understood as a number of particular cases that shared only the framework of patrimonial succession within the dynastic family. Within that context, such measures as anticipatory association were employed, varied, discontinued, or never used at all, according to the circumstances of the particular family or generation.⁶⁸

FROM THE EARLY TWELFTH CENTURY, however, the higher nobility tended to practice anticipatory association less often than previously. The change was undramatic but is discernible. The family of Blois and Champagne, which in earlier generations had often used the measure, no longer did so after the end of the eleventh century.⁶⁹ The general trend was more complex. Some families that had practiced the measure in isolated instances in the eleventh century did so again in the twelfth century; others did not use it again; still others, who had not used it earlier, did so for the first time.⁷⁰ But overall there was a decrease. Although at least one case of the practice is recorded in France for every decade of the twelfth century, the usage appears to have been increasingly exceptional. Significantly fewer cases of it are recorded from the reigns of Louis VII or Philip Augustus than from that of Philip I, despite the far greater documentation for the later period. In addition, there are geographical and chronological contrasts: while in the time of Philip I the practice appeared simultaneously in several regions, under Philip Augustus it was rare, with one case recorded for one region in a given decade but the next instance far distant and perhaps ten years later.⁷¹ The decline in the use of the

⁶⁸ A case in point is the line of the counts of Meulan, whose successional arrangements in the twelfth century varied with each generation; see Depoin, *Cartulaire de l'abbaye de Saint-Martin de Pontoise*, 315–16, 320–24; and Émile Houth, “Robert Preud’homme, comte de Meulan et de Leicester (8 avril 1081–5 juin 1118),” *Bulletin philologique et historique (jusqu’à 1610) du Comité des travaux historiques et scientifiques* (1963), 801–04, “Galeran II, comte de Meulan: Catalogue de ses actes précédé d’une étude biographique,” *ibid.* (1960), 628–29, 645, 638, and “Catalogue des actes de Robert II, comte de Meulan,” *ibid.* (1961), 502–04.

⁶⁹ The last case recorded for this line was the association of Stephen-Henry with Theobald III; see note 24, above.

⁷⁰ For families that had practiced anticipatory association earlier and now did so again, note the counts of Clermont-en-Auvergne and of Flanders; see *HP*, 16: 112; Étienne Baluze, *Histoire généalogique de la maison d’Auvergne*, 2 (Paris, 1708): “Preuves,” 249–50, 254, 256, etc.; and notes 55, 24, 50, above. The intended succession to Robert II of Meulan is probably another such case; see notes 40, 24, above. The evidence is uncertain, but there may have been such a case also in the county of Beaumont-sur-Oise; see Louis Douët-d’Arcq, *Recherches historiques et critiques sur les anciens comtes de Beaumont-sur-Oise du XI^e au XIII^e siècle* (Amiens, 1855), “Preuves,” 25; and note 24, above. But the counts of Mâcon and of Burgundy did not continue anticipatory association into the twelfth century; see notes 23–24, above. Yet the counts of Ponthieu, Anjou, and Alençon did not practice anticipatory association until the 1100s; see notes 31, 40, 45, above. The subscription of Hugh Britto as count of Ramerupt in 1101, during the lifetime of his father Andrew, signifies another such case; Luc d’Achery, ed., *Spicilegium, sive Collectio veterum aliquot scriptorum*, 3 (Paris, 1723): 437.

⁷¹ From the time of Philip I, shared rule appears among the dukes of Normandy, the counts of Flanders, Blois, Valois (probably twice), Meulan, Nevers, La Marche, Périgord, Anjou (twice), Auvergne, and Ramerupt, and the viscounts of Chartres; see notes 24, 28, 45, 55, 70, above. The viscounty of Bourges also had shared rule, although under different circumstances; see page 918, above. Another instance (in Beaumont-sur-Oise) ended at the time of Philip’s consecration; see note 24, above. From the reign of Louis

measure signaled by the Capetians was thus a wide-spread phenomenon that occurred in the nobility over the course of several generations.

No corresponding changes in the outline of baronial successions appeared during the same time. Rather, the forms used to effect these arrangements were altered to the degree that anticipatory association was employed less often than before. Partial explanations for the decline of anticipatory association may be proposed: the relative decrease in baronial violence since the eleventh century, the greater frequency of intervention by the Capetian overlords in cases of disorder, the enhanced rights of primogeniture in successions. Objections may be raised to all of these—and exceptions cited—but on balance each of them contributed to increased order in the society. In most cases, the means by which successions were arranged are not known; only the results are recorded. Rarely, there is proof of the use of the very old practice of oral dispositions by the father.⁷² In other cases, few of which can be linked to traditions of anticipatory association, the increasing use of written testaments is recorded from the reign of Philip Augustus.⁷³

The change in usages is best documented for the kings. On the eve of their departures for crusades—that is, in circumstances in which earlier kings and magnates had associated their heirs—Philip Augustus in 1190 and Louis VIII in 1225 made written testaments.⁷⁴ These testaments, however, did not prescribe, but presupposed, the succession of the eldest son. The will of Philip

VII, the only instances I have found are those from the counties of Ponthieu, Flanders, Soissons, Auvergne, and, perhaps, Beaumont; see notes 31, 46, 50, 70, above. From the reign of Philip Augustus, the counts of Dreux, Meulan, Auvergne, and Alençon practiced shared rule; see notes 40, 70, above. Doubtless these lists are incomplete, but they are sufficient to indicate the trend. Note, too, that the reigns considered were of almost equal duration: forty-eight years for that of Philip I, forty-three years each for those of Louis VII and Philip Augustus. The span of time for the use of the measure indicates its relative currency among the higher nobility: in the time of Philip I, it was used by the houses of Normandy (1065/66–87), Meulan (1056–68?), Valois (1060 or 1069–74), Blois-Champagne (1074–89), Périgord (1080–before 1114), La Marche (1086–88), Flanders (1086–93), etc.; under Philip Augustus, by contrast, it is found in Dreux (1184–88), Meulan (1183 or 1189–90), Auvergne (1199–1230?), and Alençon (1205–before 1215). The generalizations advanced here about the gradual decline in anticipatory association among the higher nobility may not be applicable, however, to similar measures used by local aristocrats. From 1195 to 1200, for example, both father and son called themselves lord of Bazoches (Aisne); Newman, *Les seigneurs de Nesle en Picardie*, 1: 127 n. e. For similar arrangements in the family of Vernon (Eure), see note 59, above.

⁷² For one such case, see Thomas K. Keefe, "Geoffrey Plantagenet's Will and the Angevin Succession," *Albion*, 6 (1974): 266–74.

⁷³ The early forms of these procedures varied. The major testamentary provisions of Count Robert II of Meulan were contained in the marriage contract of his heir; see note 40, above. The testament of Ralph I of Coucy (1190) was produced after consultation between Ralph and his vassals and appears as a record of the decisions then reached; André Duchesne, *Histoire généalogique des maisons de Guines, d'Ardres, de Gand, et de Coucy* (Paris, 1631), "Preuves," 353–54. That of Guichard of Beaujeu was a personal instrument, the authority of which lay in the written document, which Guichard caused to be authenticated by the seal of the future Louis VIII as well as his own and by the citation of five of his knights as witnesses; Marie-Claude Guigue, "Testament de Guichard III de Beaujeu (18 septembre 1216)," *Bibliothèque de l'École des Chartes*, 18 (1857): 161–67. Some testaments provided for bequests to churches and to favored retainers while leaving the devolution of the lands either to the rule of custom or to regulation under a separate document; see, for example, the testament of Alice, lady of Venisy, from 1220; Quantin, *Recueil de pièces pour faire suite au Cartulaire général de l'Yonne*, no. 245. It is not that testaments were a new practice in the time of Philip Augustus, nor at all a universal one, but rather that their use was more frequent than it had been earlier. For one older example, and another form, see the announcement by Louis VII in 1157–58 of the testamentary dispositions of Count Ivo of Soissons; Newman, *Les seigneurs de Nesle en Picardie*, 2: no. 30. Other earlier examples and variations could be cited.

⁷⁴ Delaborde, *Recueil des actes de Philippe Auguste*, 1: no. 345; and *LTC*, 2: no. 1710. Also see, for Philip's testament of 1222, *LTC*, 1: no. 1546.

Augustus from 1190 established a regency to administer the kingdom during his absence and, in the event of his own death, until the future Louis VIII came of age. That of Louis VIII referred to the heir as "our son who succeeds us in the kingdom" and chiefly regulated the assignment of apanages to the cadets, pious donations, and other matters. There is no doubt that these kings were concerned about their successions. Philip Augustus had refused to go on crusade before the birth of his heir; and, since his sons were all minors, on his deathbed Louis VIII secured oaths from the attendant magnates promising the immediate consecration of his eldest surviving son or that of the second son in the event that the eldest died before he could be crowned.⁷⁵ In principle, the succession of the eldest son was assumed and was left to the sanction of custom. Other functions formerly met through anticipatory association were effected by other means. Thus, the support of the heir who came of age during his father's lifetime was provided from the revenues of holdings assigned to him, in the case of Louis VIII from his maternal inheritance plus additional properties given him by his father. The dower for Louis's wife was provided from some of those same lands.⁷⁶ At the death of Louis VIII, his heir was too young for such needs to have arisen, but the same situations were met through similar arrangements in the reign of St. Louis.⁷⁷

What emerges, therefore, is not a sudden shift in institutions but rather a shift in preferences among both received and current usages. In broadest terms, a shift from premises of coseigniory or condominium of son with father to one of full lordship for the father and guaranteed rights of succession for the eldest son is detected, although even in this no more than a change in stress, or in the degree of stress, may be argued. With reference specifically to the Capetians, only very general conclusions may be advanced. If by the reign of Philip Augustus the throne was clearly hereditary, it has yet to be determined by what time it became so. The association of Philip Augustus with Louis VII appears to have been occasioned by special circumstances and devoid of clear constitutional significance. In view of the successional arrangements of the barons, it is questionable whether earlier instances of anticipatory association in the Capetian dynasty had the meaning previously imputed to them and whether the throne was not hereditary in the eleventh century as well. It is evident, however, that the direct evidence for anticipatory association as an index of the strength or weakness of hereditary claims will not bear the weight that traditional historiography has placed upon it.

⁷⁵ For Philip Augustus, see Rigord, *Gesta Philippi Augusti*, in Delaborde, *Oeuvres de Rigord et de Guillaume le Breton*, 1: 48. For Louis VIII, see *LTC*, 2: nos. 1811–12. Also see *LTC*, 2: 1823–28; and Charles T. Wood, *The French Apanages and the Capetian Monarchy, 1224–1328* (Cambridge, Mass., 1966), 15–16.

⁷⁶ Charles Petit-Dutaillis, *Étude sur la vie et le règne de Louis VIII (1187–1226)* (Paris, 1894), 205–06; Martène and Durand, *Veterum scriptorum et monumentorum amplissima collectio*, 1090; and *LTC*, 2: no. 2562.

⁷⁷ *LTC*, 4: nos. 5489, 5497, 3: nos. 4192, 4412, 4434. For the position of the heir with respect to the throne during the reign of St. Louis, see Louis Carolus-Barré, "Le prince héritier Louis (1244–1260) et l'interim du pouvoir royal de la mort de Blanche de Castille (novembre 1252) au retour de saint Louis en France (juillet 1254)," *Comptes-rendus de l'Académie des Inscriptions et Belles-Lettres* (1970), 588–95; and Wood, *French Apanages and the Capetian Monarchy*, 16 n. 23.

The *Miles Literatus* in Twelfth- and Thirteenth-Century England: How Rare a Phenomenon?

RALPH V. TURNER

Must we assume that medieval administrators were always accompanied by clergymen to read to them, write their letters, and figure their accounts? A misconception about the Middle Ages that dies hard is that laymen were by definition illiterate and that the Church alone could provide kings and princes with professional administrators. The growth of a literate laity is usually associated with the rise of towns and tradesmen, not with the nobles and knights of the countryside.¹ T. F. Tout, for example, assumed that the *miles literatus* was a “rare phenomenon” in England even as late as the mid-thirteenth century. Like too many others, he presupposed the static character of medieval life, unaware that the lives of the nobility changed sharply after about 1100. And the myth of lay illiteracy has survived despite the challenge of studies published as long ago as the 1930s.² True, the works of both James

I wish to thank Michael Clanchy of Glasgow University and Edward J. Kealey of Holy Cross College for their helpful comments. I also wish to thank Eleanor Rathbone for allowing me to consult her unpublished dissertation, “The Influence of Bishops and Members of Cathedral Bodies in the Intellectual Life of England, 1066–1216” (University of London, 1935).

¹ The statements found in textbooks on the history of Western civilization are less surprising than those found in medieval texts: “But reading, which fills so many hours for the modern man of leisure, was literally a closed book to most of the medieval nobility”; Wallace K. Ferguson and Geoffrey Bruun, *A Survey of European Civilization*, 1 (4th ed., Boston, 1969): 221. “Since the elder son of nobility, who was to inherit the lands, was ordinarily occupied with the long, arduous training in the use of weapons required for knighthood, he usually had no formal education to speak of”; William Langer *et al.*, *Western Civilization*, 1 (2d ed., New York, 1975): 314. “The education of the noble youth consisted of little else than training for the profession of arms”; Robert S. Hoyt, *Europe in the Middle Ages* (2d ed., New York, 1966), 303. “Neither nobles nor peasants found it useful to educate their children academically,” but “the sons of medieval merchants were taught to read and write Latin”; Jeffrey Burton Russell, *Medieval Civilization* (New York, 1968), 494. “Schooling was peculiarly an urban phenomenon. . . . Unlike the nobles, a high proportion of tradesmen could read”; Charles T. Wood, *The Quest for Eternity: Medieval Manners and Morals* (Garden City, N.Y., 1971), 113–14. And histories of education repeat the same clichés: “The nobility had little concern with academic learning in any form. . . . The warrior knight became the aristocratic ideal and his training in military accomplishments had little concern with literacy”; James Bowen, *A History of Western Education* (London, 1975), 21, 25–26. Carlo M. Cipolla has recognized that around 1100 educational opportunities increased, but he has also linked this change to the *bourgeoisie*: “Two cultures developed side by side: an urban culture that was essentially literate, and a rural culture essentially illiterate”; Cipolla, *Literacy and Development in the West* (Harmondsworth, Middlesex, 1969), 55, and also see *ibid.*, 41–44, 55–56.

² Marc Bloch, *La société féodale*, 2 vols. (Paris, 1939–40), translated by L. A. Manyon as *Feudal Society*, 2 vols. (London, 1961), 1: 79–80; V. H. Galbraith, “The Literacy of the Medieval English Kings,” *Proceedings of the British Academy*, 21 (1935): 201–37; James Westfall Thompson, *The Literacy of the Laity in the Middle Ages* (Berkeley and Los Angeles, 1939); and Lynn Thorndike, “Elementary and Secondary Education in the

Westfall Thompson and V. H. Galbraith chiefly considered the education of princes and great nobles, not simple knights; and Galbraith concluded that a significant level of lay literacy arose only when the vernacular pushed Latin aside. Yet Thompson was able to suggest that "the English nobility of the high Middle Ages were more familiar with Latin than is commonly assumed."³

Recent research in Anglo-Norman and Angevin royal administration reveals that Thompson's speculation was close to the mark. The Angevin kings were not as dependent upon the Church for their civil servants as has been thought. Those who did the day-to-day work that made Angevin government so effective were largely laymen, and they came chiefly from the knightly class, not the *bourgeoisie*. Henry II and his sons made increasing use of written records in their governments; and a process of "bureaucratization" was clearly underway. This reliance upon the written record can be seen in the writs, pipe rolls, plea rolls, feet of fines, and chancery enrollments. The value of written records was soon appreciated by those outside the government. By the mid-thirteenth century, if not earlier, manorial bailiffs maintained written accounts, and even villeins kept charters to record their sales and purchases of small parcels of land.⁴

Although sheriffs, barons of the exchequer, justices of the bench, and other officials may have employed scribes to do their writing for them, that is not necessarily a sign of their illiteracy. Reading and writing were not yet linked together, as they are today. Writing was a specialized craft not easily mastered—a point illustrated in an earlier age by Einhard's account of Charlemagne's futile efforts to learn to write long after he had learned to read. And in the twelfth century even monastic houses hired scribes for routine secretarial work.⁵ Nevertheless, the royal officials needed to be able to read the writs and rolls for themselves. Those two puncturers of myths in English medieval history, H. G. Richardson and G. O. Sayles, have been busy here, seeking to establish "that laymen engaged in public administration involving the use of written documents were sufficiently literate for the task."⁶ Their essay points up the need for a closer look at the problem of the *miles literatus* in twelfth- and thirteenth-century England.

Since it is unlikely that all of the laymen in the royal government can have

Middle Ages," *Speculum*, 15 (1940): 400–08. For Tout's assumptions, see his *Chapters in the Administrative History of Medieval England*, 6 vols. (Manchester, 1920–37), 1: 205, 288 n. 2, 3: 202. On the changing character of medieval society around 1100, see Bloch, *Feudal Society*, 2: 422.

³ Galbraith, "Literacy of Medieval English Kings," 225; and Thompson, *The Literacy of the Laity in the Middle Ages*, 180. But K. B. McFarlane, unlike Galbraith, found evidence for a continued sound grounding in Latin among the aristocracy for the late Middle Ages; McFarlane, *The Nobility of Later Medieval England* (Oxford, 1973), chap. 6: "The Education of the Nobility in Later Medieval England."

⁴ M. M. Postan and C. N. L. Brooke, eds., *Carte Nativorum*, Northampton Record Society (Northampton, 1960), xxxviii, xliii; and Dorothea Oschinsky, *Walter of Henley and Other Treatises on Estate Management and Accounting* (Oxford, 1971), 64.

⁵ Einhard, *Vie de Charlemagne*, ed. and trans. Louis Halphen (Paris, 1923), 29–30; and David Knowles, *The Monastic Order in England, 943–1216* (Cambridge, 1953), 520.

⁶ Richardson and Sayles, *The Governance of Mediaeval England from the Conquest to Magna Carta* (Edinburgh, 1963), 277. And more generally, see *ibid.*, chap. 15: "Statecraft and Learning."

been unlettered in Latin, answers to three questions must be sought: Did laymen hold government posts that required literacy? What is the evidence for laymen literate in Latin? And where did such laymen learn their letters? It may not be possible to answer these questions in a fully satisfying fashion, for little direct evidence for education of the laity survives from before the fourteenth century. But, even though answers must be tentative and based in part on speculation, it is long past time to raise the questions.

THE MADDENINGLY IMPRECISE USE of the term *clericus* in Latin ("clerk" in English) presents a semantic obstacle in trying to answer these questions. When authors—medieval or modern—use the term, do they mean it in the religious sense of "cleric," one who is in holy orders or at least tonsured? Or do they mean it in the bookish sense of an accountant, secretary, scribe, or scholar? According to Charles du Cange, in the twelfth century the terms *clerici* and *scholares* were not yet synonymous. Leona Gabel has further shown that in the time of Henry I the term *clericus* meant "a clerk in holy orders"—that is, presumably in major orders—and that by the reign of Henry III the term was also applied to those who had first tonsure; by the fourteenth and fifteenth centuries, it applied to all who could demonstrate their literacy.⁷ It is paradoxical that, in those centuries when more and more laymen were gaining an ability to read, literacy should have come to stand as the legal test for clerical status. To extend the jurisdiction of its courts, the Church encouraged a broad definition of clerical status and readily tonsured those who sought it.⁸ The possibility of gaining benefit of clergy in late medieval England by reading a verse of Scripture has doubtless contributed to the modern myth of the illiteracy of the laity throughout the Middle Ages.

When one became a *clericus* in the religious sense is significant for defining the status of students in the Middle Ages. Scholars have long assumed that students in the schools were *clerici*—tonsured, in clerical garb, and subject to the ecclesiastical courts, whether or not they were in one of the seven major orders of the Church. M.-M. Davy has demonstrated that before 1200 students, at least in Paris, were not necessarily considered members of the clergy, and only in that year did they fall fully under canon law and the Church courts.⁹ Students' clerical status, assumed only because of their student status, did not commit them to ecclesiastical careers upon completion of their studies. According to canon law, tonsuring was not an irreversible process, and it alone did not admit one to minor orders, although a candidate was usually tonsured at the time of his admission to the first of the minor orders.

⁷ Du Cange, *Glossarium mediae et infimae latinitatis*, 10 vols. in 11 (Paris, 1883–87), 3: 367–70; and Gabel, *Benefit of Clergy in England in the Later Middle Ages* (Northampton, Mass., 1928–29), 62–65.

⁸ M.-M. Davy, "La situation juridique des étudiants de l'Université de Paris au XIII^e siècle," *Revue d'histoire de l'Église de France*, 17 (1939): 300–04.

⁹ *Ibid.*, 300. But see Hastings Rashdall, *The Universities of Europe in the Middle Ages*, ed. F. M. Powicke and Alfred B. Emden, 1 (2d ed., Oxford, 1936): 393–95; and Margaret Deanesly, "Medieval Schools to ca. 1300," in J. B. Bury et al., eds., *Cambridge Medieval History*, 5 (Cambridge, 1929): 767–68.

Nothing prevented those who actually took minor orders from later choosing a secular occupation and living a secular life, even marrying. They could then be “clerks” in the bookish sense—though no longer in the religious sense—and serve as notaries or “clerical odd-job men” to draw up legal documents.¹⁰ Many of the *milites literati* may fall into such a category: men who had once taken minor orders but who subsequently devoted themselves entirely to worldly work. Only those who were in major orders—subdeacons or higher—were likely to find permanent careers in the Church, holding benefices with care of souls.

Another word that wants a definition in keeping with medieval usage is *literatus*. What could be more confused than Matthew Paris’s description of Paulin Piper, one of Henry III’s royal officers, as “quidam miles literatus sive clericus militaris”?¹¹ Du Cange has noted that *literatus* was a synonym for *clericus* and was also used in monastic writings to distinguish choir monks from lay brothers or *conversi*.¹² Clearly, medieval writers often meant more by the term than merely the ability to read. A more accurate translation in many cases might be “learned”—that is, soundly grounded in classical literature.¹³ Critics of ecclesiastical officers often called them *illiterati*, which certainly did not mean the inability to read a text.¹⁴ For example, an opponent of Ralph Neville, a thirteenth-century bishop of Chichester and royal chancellor, called him a “curialem . . . et illiteratum.”¹⁵ Royal administrators may have been then *illiterati* in the Latin of their contemporaries because of their ignorance of rules of classical rhetoric, but not “illiterate” in today’s English usage. A *miles literatus*, a “learned knight,” was rare enough to arouse comment by a chronicler, but not a knight who was merely literate in today’s sense. There were, then, three levels of literacy in the twelfth and thirteenth centuries: (1) the professional man of letters; (2) the cultivated amateur; and (3) the pragmatic reader. The adjective *literatus* applies to the first two but not the third.¹⁶ Most knights were at least pragmatic readers, functional literates in today’s terms, capable of handling simple Latin as a tool in their many tasks of government.

¹⁰ R. R. Bolgar, *The Classical Heritage and Its Beneficiaries from the Carolingian Age to the End of the Renaissance* (Cambridge, 1954), 416 n. 195; “Tonsure,” *New Catholic Encyclopedia*, 14 (1967): 199; Rashdall, *Universities of Europe in the Middle Ages*, 394–95; and H. G. Richardson, “The Oxford Law School under John,” *Law Quarterly Review*, 57 (1949): 335. Gervase of Tilbury was one cleric, a canonist, who sought a secular career and eventually married; H. G. Richardson, “Gervase of Tilbury,” *History*, 46 (1961): 114. Gervase of Chichester, one of Becket’s *eruditi*, complained of those who abandoned clerical orders in order to take secular posts; Beryl Smalley, *The Becket Conflict and the Schools* (Oxford, 1976), 226.

¹¹ Matthew Paris, *Chronica majora*, ed. H. R. Luard, Rolls Series, no. 57, 7 vols. (London, 1872–84), 5: 242.

¹² Du Cange, *Glossarium mediae et infimae latinitatis*, 5: 127.

¹³ On use of the terms in fourteenth-century England, see McFarlane, *The Nobility of Later Medieval England*, 235.

¹⁴ For the problem of defining literacy and illiteracy, see Cipolla, *Literacy and Development in the West*, chap. 1.

¹⁵ For this epithet of Simon Langton, see Matthew Paris, *Chronica majora*, 3: 207. In the eleventh century, the prior of Bec was also described as an *illiteratus*; John A. Giles, ed., *Vita Lanfranci*, 1 (Oxford, 1844): 281–83.

¹⁶ For the development of this classification, see M. B. Parkes, “Literacy of the Laity,” in D. Daicher and A. Thorlby, eds., *The Medieval World* (London, 1972), 555–77.

BY THE TIME OF HENRY II, royal government was growing rapidly, expanding its scope of activity, and requiring more officers. The king continued to depend upon the clergy for many of his servants. The office of treasurer traditionally went to a cleric until 1340, when it was first given to a knight. A cleric also always held the office of chancellor until 1340. Since the chancellor was often a bishop, his secretaries were clerics, in many instances members of his episcopal household. Indeed, clerics continued to play prominent roles in English government until the fall of Thomas, Cardinal Wolsey in the sixteenth century. Medieval monarchs may have felt that clerics made more pliable servants—ones less likely to put family interests first, less likely to build up landed inheritances. But clerical civil servants were almost as susceptible to these temptations as laymen. Thus, medieval kings relied mainly upon clergymen to staff their secretariats neither because they were celibate nor because they were the only available literates but because they could hold ecclesiastical benefices, which made it possible to provide their livings—their pay—at the Church's expense.

The lay element in royal administration, however, increased after the mid-twelfth century, and perhaps earlier. The sheriff was generally a layman, first from among the baronage and later from among the knights.¹⁷ He had lay assistants, undersheriffs, who had about the same social rank as the estate stewards of great barons. The novelty of Henry I's policy of raising new men "from the dust" to help him govern may be questioned, but there can be no dispute that he depended heavily upon laymen in governing England. William T. Reedy has identified eleven itinerant justices under Henry I, all of whom appear to have been laymen; and C. Warren Hollister has more recently described Henry I's *curiales*, many of whom were laymen.¹⁸ Later, one of Henry II's judicial experiments was to assign five members of his household—two clerics and three laymen—to hear pleas.¹⁹ The first of the "chief justiciars," Roger of Salisbury, was an ecclesiastic, but under Henry II and his sons the post frequently fell to a layman. Seven of the thirteen justiciars who held office from the time of Henry I until the extinction of the position under Henry III were laymen, and four of the clerics served under Richard I. The chamberlains of Henry I and of Henry II and his sons were always laymen of knightly rank, and the chamber staff included more laymen than clerics. The stewards of the royal household were also laymen, even though they had to be able to keep accounts; and one of them—Henry III's steward, John of Lexington—was praised by the chroniclers for his learning.²⁰

¹⁷ Occasionally a bishop held a shrievalty. For examples, see Public Record Office, *Lists and Indexes*, vol. 9: *List of Sheriffs for England and Wales from the Earliest Times to A.D. 1831* (London, 1898).

¹⁸ Reedy, "The Origins of the General Eyre in the Reign of Henry I," *Speculum*, 41 (1966): 698–704; and Hollister and John W. Baldwin, "The Rise of Administrative Kingship: Henry I and Philip Augustus," *AIIR*, 83 (1978): 867–90 (above). Also see—for Geoffrey of Clinton, Henry I's chamberlain—R. W. Southern, *Medieval Humanism and Other Studies* (New York, 1970), 214.

¹⁹ William Stubbs, ed., *Gesta Regis Henrici Secundi*, 1, Rolls Series, no. 30 (London, 1867): 207–08.

²⁰ According to Matthew Paris, John of Lexington was a man of great authority and knowledge (*scientia*), and, according to the Chronicle of Burton, he was a layman skilled in civil and canon law; Matthew Paris, *Chronica majora*, 5: 384; and H. R. Luard, ed., *Annales Monastici*, 1, Rolls Series, no. 36 (London, 1864): 345. Even the stewards of private households kept accounts of daily expenses. The earliest surviving one dates

More significant than those who held some formal title in Angevin government were the *familiares regis* or *curiales*, men of diverse social standing who had the king's confidence and who were ready to serve him in any capacity: as sheriffs, custodians of castles, barons of the Exchequer, itinerant justices, ambassadors abroad, even generals. Their work was so wide-ranging that it is impossible to list them under any one branch of government.²¹ Although their names appear frequently in the records, not much is known about them. No one has made a detailed study of the *familiares* of Henry II, but many—perhaps most—of them were laymen.²² Sidney Painter has identified five trusted agents who were among King John's *familiares*, men below the great officers of state but almost their equals in administrative responsibility: William Marshal, William de Braose, Peter des Roches, Hugh Bardolf, and William Briwerre.²³ Of these five key figures, only one—Peter des Roches—was in clerical orders.

Some of the "men of all work" can be identified through the feet of fines, records of agreements made at the exchequer, on eyres, or later before the bench at Westminster. Many of the Angevin kings' *familiares* spent part of their time as judges; their names thus survive as witnesses to the final concords. A significant number of names on these records are those of laymen. In the years before 1179, when a group of justices was appointed to hear pleas, Henry II periodically sent out groups of itinerant justices, many of whom were also barons of the exchequer. Of over forty men, some can only be identified by their names on the feet of fines, but, of those about whom more is known, at least twenty-six were laymen and only five were clerics. Between 1179 and 1189, forty-eight different justices witnessed fines made at Westminster, and twenty of these were laymen. Raoul C. van Caenegem has calculated that of all of Henry II's judges only about a dozen, "not even ten percent of the total force of justices," had the title *magister*, which is indicative of formal study at a cathedral school.²⁴ During the reign of Richard I, some sixty men served on the bench. Certain names recur often enough that those men can be considered professional royal servants specializing in the work of justice, even though it is not yet appropriate to speak of a professional judiciary. Five of the ten professionals who served Richard were laymen. Of the roughly ninety

from 1265, that of the countess of Leicester; see Margaret Labarge, *A Baronial Household of the Thirteenth Century* (New York, 1965), 13, 60.

²¹ W. L. Warren, *Henry II* (Berkeley and Los Angeles, 1973), 310; and Doris M. Stenton, "England: Henry II," in Bury *et al.*, *Cambridge Medieval History*, 579–80. For one very precise definition of Henry II's *curiales*, see Hollister and Baldwin, "The Rise of Administrative Kingship: Henry I and Philip Augustus," 887, table 3 (above).

²² For the names of some of them, see Warren, *Henry II*, 308–14; and Stenton, "England: Henry II," 579–81.

²³ Painter, *The Reign of King John* (Baltimore, 1949), 70–71.

²⁴ Van Caenegem, *The Birth of the English Common Law* (Cambridge, 1973), 23. For the justices of this period, see Doris M. Stenton, ed., *Pleas before the King or His Justices, 1198–1202*, Selden Society, 4 vols. (London, 1952–66), 3: app. 1: "The Development of the Judiciary, 1100–1215," xlvii–ccxciv. Of the forty-eight justices of the years 1179–89, another twenty were ecclesiastics, including eight bishops, while the status of eight is unknown. For biographical details, unfortunately not always reliable, see Edward Foss, *The Judges of England* (London, 1848), vols. 1–2.

men who assisted King John as judges, many were great men of the kingdom sitting briefly with the king or local notables with temporary appointments as itinerant justices, but fifteen regularly served as royal judges at Westminster. Eleven of these fifteen were laymen. During the minority of Henry III, four of the seven justices sitting with regularity at Westminster were laymen.²⁵ Frederick William Maitland stressed the ecclesiastical character of the English judiciary in the age of Glanvill, and T. F. T. Plucknett agreed that the English judiciary was largely clerical in composition until 1300.²⁶ But a look at the lists of judges under Henry II, his two sons, and his grandson shows a large lay contingent a century earlier. England, moreover, was not alone in having numbers of laymen learned in the law. The Latin Kingdom of Jerusalem produced a remarkable group of jurists who were laymen of noble rank and acquainted with a broad range of legal literature.²⁷

WERE THESE LAYMEN who carried out so many tasks of government in twelfth-century England all illiterates? The constantly increasing number of written documents with which they had to cope implies at least their pragmatic literacy. Yet the traditional view is that they were unable to read and that they relied upon clerks to maintain the necessary records.²⁸ V. H. Galbraith, an expert on medieval public records, maintained that the clergy monopolized record keeping as long as the government's language remained Latin and that only a return to the vernacular broke that monopoly.²⁹ But as Thompson, Richardson and Sayles, and others have pointed out, laymen learned in Latin were not unknown in the years following 1100. References to literate laymen can occasionally be found in chronicles; other evidence is less direct. Galbraith's own work on the literacy of the English monarchs supplies such indirect evidence, for the education of the magnates' children must have paralleled that of the royal heirs. Galbraith pointed out that the kings before 1100 were largely illiterate; in the twelfth and thirteenth centuries princes were taught to read Latin; and by the late Middle Ages they learned French and English, as first French and then English replaced Latin as the language of government.

Henry I *Beauclerc* has an exaggerated reputation for learning that did not arise until long after his death. Charles W. David has made a detailed study of the sources of the king's reputation and concluded, "It cannot be doubted

²⁵ Those who served at least ten terms on the bench are counted as professionals. Possibly, the relatively large number of lay justices who served under John is in part due to John's quarrel with Pope Innocent III, for clerics could not serve an excommunicate king. For Henry III's justices, I consulted an unpublished list compiled by the late C. A. F. Meekings, who kindly allowed me to use it.

²⁶ Frederick Pollock and F. W. Maitland, *History of English Law before the Time of Edward I*, 1 (2d ed., Cambridge, 1898): 133-35, 205; and Plucknett, "The Place of the Legal Profession in the History of English Law," *Law Quarterly Review*, 48 (1932): 328-40.

²⁷ Jonathan Riley-Smith, *The Feudal Nobility and the Kingdom of Jerusalem, 1174-1277* (London, 1973), chap. 6: "A School of Feudal Jurists," 121-44.

²⁸ Tout, for example, stated, "As in the exchequer, knights [of the chamber] could only keep account by tallies, and could not write letters at all. . . . There was, therefore, an imperative need in the camera for a staff of experts in writing and finance"; *Chapters in the Administrative History of Medieval England*, 1: 116.

²⁹ Galbraith, "Literacy of the Medieval English Kings," 225, 229-30.

that Henry had his opportunity to learn Latin, and that he did acquire a considerable, though by no means complete, mastery of the language. But it is equally certain that his great fame as a learned king is the product of a later age, not of the age in which he lived."³⁰ Henry's first wife, Matilda, was a woman with a strong interest in letters, which she had learned under an aunt who was a nun. And their court was clearly a cultural center, where youths such as Henry's natural son, Robert of Gloucester, and his nephew, Stephen of Blois, as well as the sons of other aristocrats came to learn Latin letters.³¹

Henry II provides evidence for the greater emphasis given to studies by the mid-twelfth century, for he had an excellent education. He attracted to his court a brilliant group of writers, who commented favorably upon his learning in their works. And he made certain that all of his sons mastered the elements of Latin. Each boy had a *magister* or *preceptor*, one of the *familiares regis*, charged with general supervision of his upbringing. The *preceptor* of the young Henry was Thomas Becket, who may not have taken his responsibility too seriously; he left the boy's formal schooling to tutors.³² Peter of Blois wrote a letter in which he expressed the fear that the knightly side of the young Henry's education was being emphasized at the expense of the liberal arts. Certainly, the young king's career indicates that the fear was justified, for the code of chivalry weighed more heavily in motivating him than did the classics. Yet Gervase of Tilbury, who belonged to his household for several years, wrote a book of jests for him and started a second work on the assumption that he would be able to read them.³³ Richard I is usually linked in modern minds with French chivalric literature, but he was equally at ease with Latin. He knew the language well enough to correct the grammar of Hubert Walter, archbishop of Canterbury.³⁴ The youngest of Henry's sons, John, was first sent to the monks and nuns of Fontevrault; later, he was handed over to Ranulf of Glanvill, the justiciar, who served as his *magister*. King John certainly had some interest in literature, for he owned a collection of the classics and devotional works that he left for safekeeping at Reading Abbey.³⁵

³⁰ David, "The Claim of King Henry I to be Called Learned," in Charles H. Taylor and John L. LaMonte, eds., *Anniversary Essays in Medieval History by Students of Charles Homer Haskins* (Boston, 1929), 56. Galbraith agreed with David; "Literacy of Medieval English Kings," 211-12. But Thompson wrote, "It seems probable that Henry I was a better scholar, and more entitled to his title of Beauclerc, than Professor David would permit us to believe"; *The Literacy of the Laity in the Middle Ages*, 170.

³¹ M. Dominica Legge, "L'influence littéraire de la cour d'Henri Beauclerc," in Fred Dethier, ed., *Mélanges offerts à Rita Lejeune*, 1 (Gembloux, 1969): 679-87; R. H. C. Davis, *King Stephen* (Berkeley and Los Angeles, 1967), 4; and Leslie Stephen and Sidney Lee, eds., *The Dictionary of National Biography* [hereafter *D.N.B.*], 22 vols. (London, 1885-1901), *sub nom.* "Matilda" and "Robert of Gloucester."

³² For contemporary comments on Henry II's learning, see, for example, Walter Map, *De nugis curialium*, ed. M. R. James, *Anecdota Oxoniensia*, 4th ser., no. 14 (Oxford, 1914), 237-42; Gerald of Wales, *Opera*, ed. J. S. Brewer, J. F. Dimock, and G. F. Warner, 8 vols., *Rolls Series*, no. 21 (London, 1861-91), 5: 302-06, 6: 213-15, as cited in Warren, *Henry II*, 208. For the king's education, see Warren, *Henry II*, 38-39; for that of his sons, see Thompson, *The Literacy of the Laity in the Middle Ages*, 178.

³³ Letter of Peter of Blois, written in the name of the archbishop of Rouen, in Jacques-Paul Migne, ed., *Patrologiae cursus completus . . . series Latina*, 207 (Paris, 1862): cols. 210-12; and Richardson, "Gervase of Tilbury," 105. Also see Thompson, *The Literacy of the Laity in the Middle Ages*, 178.

³⁴ Gerald of Wales, *Opera*, 3: 30.

³⁵ W. L. Warren, *King John* (Harmondsworth, Middlesex, 1966), 41; Austin Lane Poole, *From Domesday Book to Magna Carta, 1087-1216*, vol. 3 of *The Oxford History of England* (2d ed., Oxford, 1955), 243; and Thompson, *The Literacy of the Laity in the Middle Ages*, 179.

When the death of John left the nine-year-old Henry III as king, responsibility for the boy's formal schooling lay with Peter des Roches, while his training for knighthood was in the charge of Philip of Aubenay.³⁶ Galbraith and Thompson, stressing the rise of French as a literary language by the thirteenth century, have tended to doubt Henry III's ability to read Latin.³⁷ But everything known of his character and interests points toward the assumption that he had mastered Latin at least as well as his fathers and uncles had.

The care that Henry I took in the education of Robert of Gloucester, his illegitimate son, shows that not just royal heirs were thought to need schooling in the early twelfth century. Robert's early education was entrusted to the bishop of Lincoln, Robert Bloet, who had a reputation as a patron of letters. His pupil grew up to become the patron of several writers, most notably William of Malmesbury.³⁸ The ideas of the earls and barons about the education of their children cannot have been different from those of their lord, the king. A son of Hugh Otwell, earl of Chester, must have gained some skill in letters, for he was a *tutor et paedagogus* at the court of Henry I. Whether he was in clerical orders is unknown.³⁹ Two great nobles who had remarkable reputations for learning were Waleran of Meulan, earl of Worcester, and his twin brother Robert, earl of Leicester. In 1118, when the twins were fourteen, they were sent to the court of Henry I for advanced schooling. The next year, when the pope and the king met in Normandy, the two boys astonished the cardinals with their dialectical skill. And the testimony of Geoffrey of Monmouth for Waleran and of Richard fitz Neal for Robert, for example, points to the learning of these two young nobles.⁴⁰ Another great noble who demonstrated his learning in Latin and likewise received his education at the court of Henry I was Brian fitz Count, lord of Wallingford, an ardent advocate of the empress Matilda. He wrote a pamphlet—which, unfortunately, no longer survives—in support of Matilda's claims against Stephen and a letter—which does survive—denouncing those who had broken their oaths and abandoned her. His letter, "though not unimpeachable in the point of Latinity, bears out his reputation as a man of some learning and acute intelligence."⁴¹

³⁶ F. M. Powicke, *King Henry III and the Lord Edward: The Community of the Realm in the Thirteenth Century*, 1 (Oxford, 1947): 9.

³⁷ Galbraith, "Literacy of the Medieval English Kings," 215; and Thompson, *The Literacy of the Laity in the Middle Ages*, 180.

³⁸ For Bloet, see Henry of Huntingdon, *Historia Anglorum*, ed. Thomas Arnold, Rolls Series, no. 74 (London, 1879), 298–300, as cited in Martin Brett, *The English Church under Henry I* (Oxford, 1975), 174–75. Brett notes that possibly another royal bastard was entrusted to Alexander, Robert Bloet's successor; *English Church under Henry I*, 175. For William of Malmesbury, see G. E. Cockayne, *The Complete Peerage of England, Scotland, Ireland, Great Britain, and the United Kingdom*, ed. V. Gibbs et al., 13 vols. (London, 1910–59), 5: 686 n. a.

³⁹ Richardson and Sayles, *The Governance of Mediaeval England*, 272–73.

⁴⁰ William of Malmesbury, *Gesta Regum Anglorum*, ed. William Stubbs, 2, Rolls Series, no. 90 (London, 1889): 482; Geoffrey of Monmouth, *Historia regum Britanniae*, ed. Jacob Hammer (Cambridge, Mass., 1951), prefatory letter; and Charles Johnson, ed., *Dialogus de Scaccario* (London, 1950), 57.

⁴¹ H. W. C. Davis, "Henry of Blois and Brian fitz Count," *English Historical Review* [hereafter *EHR*], 25 (1910): 298–99.

ALTHOUGH ELUSIVE, ISOLATED EVIDENCE of educated laymen of lesser rank survives from the first half of the twelfth century. Among the letters of Herbert, bishop of Norwich, is one written at the beginning of the century that praises the style of a lay correspondent, a certain John, who may have been a former student at the Norwich priory school.⁴² Orderic Vitalis, the Anglo-Norman chronicler, described some Norman knights who entered the house of Saint Evroul as “well versed in learning,” and he explained that some of them were able to take charge of the administration of the abbey’s business affairs. He described the knights who protected the abbey of Maule as coming often to the cloister to discuss with the monks “practical as well as speculative matters.”⁴³ In the late 1130s a Yorkshire baron, Walter Espec, possessed a copy of Geoffrey of Monmouth’s *Historiae regum Britanniae*, which he lent to another northern landholder, Ralph fitz Gilbert. Ralph’s wife, Constance, a Hampshire heiress, then lent the book to Geoffrey Gaimar, who used it in the composition of *L’estoire des Engleis*.⁴⁴ According to Gaimar, Constance was a woman of some education, for she had paid a silver mark for a *Life of Henry I* written by the historian David, and “en sa chambre sovent le lit.”⁴⁵

Sanson de Nantuil made a French translation of the Proverbs of Solomon in the middle of the century as an aid to the study of the Latin text for Roger de Condet, a boy of about twelve. The lad was the son of Robert de Condet, a Lincolnshire knight, and of Alice, who was probably a daughter of Earl Ranulf of Chester.⁴⁶ Philip, son of Earl Patrick of Salisbury, went off to Italy to study in the law schools of Bologna in the 1160s while retaining his lay status. In the late twelfth century Gilbert fitz Baderon, lord of Monmouth, had a collection of books in his castle, both French and Latin, according to the poet—Hue de Rutland—whose patron he was. A master was engaged for the grandsons of the earl of Oxford sometime around 1187.⁴⁷

By the late twelfth century, evidence that literacy had spread beyond the clergy, the monarch, and the great barons becomes easier to find. In the courtly romances it is not uncommon to see indications of literacy among noblemen—or even among ladies—where in the earlier *chansons de geste* literate laymen were regarded as something upon which to remark.⁴⁸ Gerald of Wales

⁴² Herbert de Losinga, *Epistolae Herberti de Losinga, Osberti de Clara et Elmeri Prioris Cantuariensis*, ed. Robert Anstruther, Caxton Society (London, 1846), nos. 45, 58, as cited in Richardson and Sayles, *The Governance of Mediaeval England*, 272. James W. Alexander has suggested that the layman was a former student at the priory school.

⁴³ Orderic Vitalis, *The Ecclesiastical History of Orderic Vitalis*, ed. and trans. Marjorie Chibnall, 4 vols. (vols. 2–5) (Oxford, 1969–75), 3: 118–19, 206–07.

⁴⁴ M. Dominica Legge, *Anglo-Norman Literature and Its Background* (Oxford, 1963), 28, 277.

⁴⁵ Legge, “L’influence littéraire de la cour d’Henri Beauclerc,” 683. David’s life of Henry I seems to have been in verse, set to music, and most likely in Anglo-Norman, not Latin; Antonia Gransden, *Historical Writing in England, c. 550–c. 1307* (Ithaca, N.Y., 1974), 211.

⁴⁶ Legge, *Anglo-Norman Literature and Its Background*, 38–42. Roger was born ca. 1138/39 and died before 1201.

⁴⁷ Richardson, “Gervase of Tilbury,” 105–06; Legge, *Anglo-Norman Literature and Its Background*, 85–86, 95; and Richardson and Sayles, *The Governance of Mediaeval England*, 273.

⁴⁸ For references, see Eric Auerbach, *Literary Language and Its Public in Late Latin Antiquity and in the Middle Ages* (New York, 1965), 291–93. For example, one romance, *Floir and Blancheflor*, describes its hero as able to read Latin, to write on parchment, and to read books of the pagans.

mentioned a *miles literatus* who amused himself by composing Latin verses. Gerald also apologized for the simple style of one of his books, adding that he wished to make it accessible to “laymen and princes not too well grounded in letters”—a sure indication that he thought they could at least read simple Latin.⁴⁹ In King John’s time, the sons of one baron, Richard de Umfraville, who were sent as hostages to the royal court, were accompanied by their tutor.⁵⁰ Simon de Montfort seems to have had a thorough education, as shown by his scholarly and spiritual interests throughout his life. He was so concerned about the education of his two eldest sons that he followed the early practice of sending them off to the household of a bishop, Robert Grosseteste, for instruction. Simon’s eldest son learned his letters at least well enough to write out his father’s will in his own hand, though in French, not in Latin.⁵¹

The medieval Church, fearful that clerics would be tainted by sharing in the shedding of blood, legislated continually against their participation in secular government.⁵² By the beginning of the thirteenth century, some canonists and theologians were aware that there were adequate numbers of literate laymen for service in the secular courts. Certainly that was the case in Italy, homeland of so many canon lawyers. Peter the Chanter and Robert of Courson, two theologians at Paris with “a passionate interest in practical morality,” advised secular princes to assign literate laymen to draft documents ordering sentences of death.⁵³ Moralists and satirists like Walter Map and Nigel Wireker also complained against the employment of clerics in royal government.⁵⁴ Apparently, they were confident that the king could find sufficient numbers of educated laymen to replace the clergymen. A series of diocesan and provincial councils of the English Church passed legislation aimed at giving practical meaning to the principle of clerical withdrawal from worldly affairs, particularly after the Fourth Lateran Council barred the clergy from participation in blood judgments.⁵⁵

The most abundant evidence of lay literacy comes from the careers of royal servants, who clearly had to be able to read Latin to carry out their tasks. As Richardson and Sayles have stated, “We may presume, then, that a layman who exercised an office demanding the use of written instruments was literate. . . .”⁵⁶ To support this conclusion, a look at the seven laymen who held the office of justiciar is useful. Unquestionably, an ability to read the rolls, writs,

⁴⁹ Gerald of Wales, *Opera*, 8: 310, and *Expugnatio Hibernica*, in *Opera*, 5: 207–08.

⁵⁰ Warren, *King John*, 201.

⁵¹ Margaret Labarge, *Simon de Montfort* (London, 1962), 22–23, 76; and Charles Bémont, *Simon de Montfort*, trans. E. F. Jacob (Oxford, 1930), app. A: 778.

⁵² On this question, see Ralph V. Turner, “Clerical Judges in English Secular Courts: The Ideal versus the Reality,” *Medievalia et Humanistica*, new. ser., 3 (1972): 75–98.

⁵³ John W. Baldwin, *Masters, Princes, and Merchants: The Views of Peter the Chanter and His Circle*, 1 (Princeton, 1970): 17, 185.

⁵⁴ Turner, “Clerical Judges in English Secular Courts,” 83–85.

⁵⁵ *Ibid.*, 78–79.

⁵⁶ Richardson and Sayles, *The Governance of Mediaeval England*, 274. Or, as McFarlane has written of late medieval royal officials, “We are entitled to believe those who appeared to function did so until the contrary is proved”; *The Nobility of Later Medieval England*, 229–30.

charters, and chirographs was a requirement for any occupant of that office.⁵⁷ The earl of Leicester's reputation for learning was noted in the *Dialogus de Scaccario*.⁵⁸ His colleague and successor, Richard de Luci, came from a lower level of the landed classes, yet he clearly was "an able administrator and a skilled judge"; his role in drafting the Constitutions of Clarendon provides ample evidence of his learning.⁵⁹ Ranulf de Glanvill, his successor, came from a similar social level, but he had such a reputation for learning that the *Treatise on the Laws and Customs of England* has been attributed to him from the thirteenth century forward. Recently, he has been proposed as the author of two other works: a crusading chronicle of the conquest of Lisbon and an account of an East Anglian shire-moot.⁶⁰ Earl William de Mandeville, a close friend and counsellor to Henry II, died before he could assume office. He had some interest in vernacular letters, for he was a patron of Anglo-Norman poets; possibly he was the Count William to whom Marie de France dedicated her *Fables*.⁶¹ Geoffrey fitz Peter, "one of the most learned of justiciars," came from a family of professional royal servants. He possibly began his career as clerk in the circle of Ranulf de Glanvill, then moved on to hold a number of posts, becoming the chief assistant of Hubert Walter before his own appointment as justiciar. Doris M. Stenton has speculated that he was the author of the treatise traditionally attributed to Glanvill.⁶² Like his predecessors, Hubert de Burgh came from the class that was later called "country squires." He had been chamberlain to John when the future king was count of Mortain; when John was crowned, Hubert became a royal chamberlain. Thus, he was an experienced administrator before he was named justiciar. Following his fall from Henry III's favor, when he fled to sanctuary, the king commanded that no letters should reach him and even commanded that his psalter be taken from him—surely an indication of his literacy.⁶³ Another experienced administrator, Stephen de Segrave, succeeded to the justiciarship. According to Matthew Paris, Segrave deserted the clergy for knighthood out of arrogance and sought to resume wearing the tonsure on losing royal favor.⁶⁴ No other evidence supports this story, but, if Segrave had attended school in his youth, he may once have been tonsured.

⁵⁷ Richardson and Sayles, *The Governance of Mediaeval England*, 274; and Francis West, *The Justiciarship in England, 1066-1232* (Cambridge, 1966), 45-47.

⁵⁸ Johnson, *Dialogus de Scaccario*, 57; and see page 936, above.

⁵⁹ Stenton, "England: Henry II," 578; and West, *The Justiciarship in England*, 39.

⁶⁰ On his learning, see Roger of Howden, *Chronica*, ed. William Stubbs, 2, Rolls Series, no. 51 (London, 1869): 215; and Gerald of Wales, *Opera*, 8: 258. On his authorship of the two works, see J. C. Russell, "Ranulf de Glanvill," *Speculum*, 45 (1970): 69-70.

⁶¹ Sidney Painter, "To Whom Were Dedicated the *Fables* of Marie de France?" *Modern Language Notes*, 47 (1933): 367-69, reprinted in Fred A. Cazell, ed., *Feudalism and Liberty* (Baltimore, 1961), 105-10.

⁶² F. M. Powicke, *Stephen Langton* (Oxford, 1928), 115; *DNB*, *sub nom.* "Geoffrey fitz Peter"; West, *The Justiciarship in England*, 99, 109-10; and Stenton, *Pleas before the King or His Justices, 1198-1202*, 1: 9-10. In earlier work, Lady Stenton had considered Glanvill's authorship to be "most probable"; "England: Henry II," 578.

⁶³ Michael Weiss, "The Castellan: The Early Career of Hubert de Burgh," *Vivator*, 5 (1974): 235-36; and *Close Rolls of Henry III, 1231-1234* (London, 1905), 161.

⁶⁴ Matthew Paris, *Chronica majora*, 3: 293.

There is evidence that lesser office holders in Angevin government were also literate. Certainly if the justiciar was literate, then the laymen who sat at the exchequer with him as barons or on the bench as justices must also have been literate. Sheriffs had to be capable of reading the royal writs sent to them, and their responses often required them to make a written record, returning information to Westminster *per breve suum* or *litteris suis sigillatis*.⁶⁵ One sheriff in 1159 gave a moneylender his promise to repay, purportedly written in his own hand.⁶⁶ An illiterate sheriff or custodian would have been at a great disadvantage when he presented his accounts for auditing at the exchequer. The tallies and checkered cloth used by the barons of the exchequer do not imply illiteracy on the part of those presenting accounts there.⁶⁷ Those laymen who were *familiars* of King John could not have performed their varied functions without an ability to read—Reginald of Cornhill, for example, was sheriff of Kent, chamberlain of London, custodian of mints and exchanges, collector of the fifteenth, and justice of the bench. Another valued agent was William Briwerre—sheriff, royal justice, custodian of escheats, and warden of the stanneries. He played an important part in drafting borough charters and was apparently one of the king's chief negotiators with the boroughs.⁶⁸

Several thousand knights in the counties who held no permanent administrative posts were enlisted in the work of royal government as jurors, coroners, and commissioners of various sorts. In these capacities they were brought more and more into contact with written records. Search for social status and for local political influence made them seek office in their shires, and literacy was an obvious advantage in fulfilling the functions of office. Furthermore, the business of managing their own affairs—leases, lawsuits, estate accounts, business correspondence—meant that literacy was becoming increasingly necessary for them. Richardson and Sayles have collected documents “couched in uncouth Latin” that they believe were written by twelfth-century laymen unaided by clerks.⁶⁹

By the thirteenth century, practical treatises, written for the instruction of laymen, appeared for four professions: common lawyers, estate stewards or bailiffs, conveyancing clerks, and accountants. Of course, these four fields were closely related, and an individual might concern himself with more than one of them.⁷⁰ Legal treatises and registers of writs were being compiled for

⁶⁵ William A. Morris, *The Medieval English Sheriff to 1300* (Manchester, 1927), 115, 212–22, 146–47.

⁶⁶ Hilary Jenkinson, “A Moneylender’s Bonds of the Twelfth Century,” in H. W. C. Davis, ed., *Essays in History Presented to Reginald Lane Poole* (Oxford, 1927), 190, 206.

⁶⁷ Richardson and Sayles, *The Governance of Mediaeval England*, 279–82. Tallies had the name of the person paying and the reason for payment written on them; these notched sticks continued to be cut until 1783 (and the burning of old ones may have caused the destruction of the Houses of Parliament in 1834); R. L. Poole, *The Exchequer in the Twelfth Century* (London, 1912), 86–91.

⁶⁸ Painter, *Reign of King John*, 81, 137, 147; and W. R. Powell, “English Administrative Families in the Twelfth- and Thirteenth-Centuries with Special Reference to the Cornhill Family” (B. Litt. Thesis, Oxford, 1952), 116. Briwerre witnessed forty-four of the ninety-eight borough charters issued in John’s reign; Powell, “English Administrative Families,” 109.

⁶⁹ Richardson and Sayles, *The Governance of Medieval England*, 275–77. In general, see Nicholas Orme, *English Schools in the Middle Ages* (New York, 1973); but Orme has been doubtful that literacy was widespread until the triumph of the vernacular in the reign of Richard II; *ibid.*, 33–34.

⁷⁰ Oschinsky, *Walter of Henley and Other Treatises on Estate Management*, 62.

members of the legal profession. As early as the time of King John, the plea rolls describe a professional attorney coming before the court *cum suo libro*, some sort of record he kept of the suits in which he was involved.⁷¹ By the mid-thirteenth century knights of the counties had so many public responsibilities that they needed legal expertise. Little handbooks, compilations of law tracts, legal texts, and other documents appeared in order to supply that need.⁷² The stewards of great estates in the thirteenth century were usually men of knightly rank who kept their accounts in Latin.⁷³ Evidence for their literacy comes from rentals, customals, and treatises on estate management composed for their guidance. The earliest treatises on estate management date from the middle of the thirteenth century, although they were much more common in the early fourteenth century. Among the earliest is that of Walter of Henley, written about 1285. Henley did not write his book until he became a Dominican friar, but it was based on his earlier experience: he was a knight who had received a good education, including some study of Latin, and who had served as a manorial bailiff.⁷⁴ The earliest surviving estate accounts, kept in Latin, date from the early part of the century, while the earliest treatises on accounting appeared a little later, around 1225.⁷⁵

WHERE DID THESE LAYMEN LEARN THEIR LETTERS? The usual view of knightly education is that it stressed skill in arms and courtesy learned in the household of some great noble and that the nobility looked at bookish learning with indifference, if not contempt. By the mid-twelfth century, however, warfare and chivalry were no longer adequate for the education of knights, and a third discipline—letters—was becoming essential. John of Salisbury wrote that he had supported himself at Paris by taking the children of nobles as pupils.⁷⁶ Some nobles sent their sons off to cathedrals to be schooled in bishops' households. Others sent theirs to that of the king. The courts of Henry I and Henry II included schoolmasters who gave instruction not only to the kings' children but also to other noble youths sent to court for their upbringing.⁷⁷ Numbered among such youths was an Italian, a relative or protégé of Rolando Bandinelli, the future Pope Alexander III, commended to Henry II's court by Arnulf, bishop of Lisieux. Other aristocratic households included, in

⁷¹ C. T. Flower *et al.*, eds., *Curia Regis Rolls*, 6 (London, 1933): 228.

⁷² For discussions of the compilation of Robert Carpenter, for example, made about 1260–61, see Noël Denholm-Young, "Robert Carpenter and the Provisions of Westminster," *EHR*, 50 (1935): 22–35; and C. A. F. Meekings, "More about Robert Carpenter of Hareslade," *EHR*, 72 (1957): 260–69. Meekings has stated that the "resulting book seems to be unique now" but that in "Robert's day . . . there must have been many such compilations"; "Robert Carpenter of Hareslade," 269.

⁷³ Labarge, *A Baronial Household of the Thirteenth Century*, 63; and Oschinsky, *Walter of Henley and Other Treatises on Estate Management*, 64.

⁷⁴ Oschinsky, *Walter of Henley and Other Treatises on Estate Management*, 56, 145–46.

⁷⁵ *Ibid.*, 64, 226, pipe roll of Winchester, 1208/9.

⁷⁶ John of Salisbury, *The Metalogicon of John of Salisbury*, ed. and trans. Daniel D. McGarry (Berkeley and Los Angeles, 1955), 98, lib. II, cap. x.

⁷⁷ William Stubbs, *Seventeen Lectures on the Study of Medieval and Modern History* (Oxford, 1900), 163. Orderic Vitalis told of two Norman knights—Robert II of Grandmesnill and Ralph the "Ill-tonsured"—who as early as the mid-eleventh century had gone to school; *Ecclesiastical History*, 2: 40, 76.

imitation of the royal court, a tutor or at least a chaplain who could give lessons in Latin. Hubert Walter, for example, testified that he had received his early education in the household of his uncle, Ranulf de Glanvill.⁷⁸

What opportunities were there for sons of humbler knights—those young men who did not go off to a noble household to serve as pages—to secure an education? An early twelfth-century master exaggerated when he wrote that “throughout Normandy and England, not only in the cities and castles but also in small villages, there are as many practised school-masters as there are tax-collectors and other royal officials.”⁷⁹ But opportunities for elementary and secondary education were more widely available in England than is generally recognized. The universities could not have arisen unless pupils had been prepared by a network of primary schools. There were many schools in England where boys might have studied Latin grammar and a few where they might have gained some business training, including a smattering of law. But schools were not the sole source of literacy; boys could learn to read in less formal settings. No doubt, most boys in the twelfth and thirteenth centuries learned their Latin from parish priests, the priests’ assistants, parish clerks, or their own mothers.⁸⁰

In pre-Conquest England monastery schools had been open to boys who were not destined for the religious life, even though the schools’ main duty was to train children dedicated to the cloister. Most likely, they accepted the sons of some of the leading families of the shire to study alongside the novices. Norman monasticism had no such tradition, except possibly at Bec, but some of the English abbeys continued to accept sons of important neighbors after the Conquest.⁸¹ Monasteries in the twelfth century still provided instruction for some boys who were not destined for the monastic life; Richard fitz Neal, for example, received his early education at the monastery of Ely, Thomas Becket at the priory of Merton, and Robert of Beaumont at the abbey of Abingdon. As late as the mid-thirteenth century, boys from neighboring gentry families were boarded at monasteries to learn their letters in the abbot’s household; and some houses instructed poor youths—almonry boys—for charity’s sake and for their service as acolytes at the daily masses.⁸²

⁷⁸ Frank Barlow, ed., *The Letters of Arnulf of Lisieux*, Camden Society, 3d ser., no. 61 (London, 1939), 20–21, no. 15; and Charles R. Young, *Hubert Walter, Lord of Canterbury and Lord of England* (Durham, N.C., 1968), 4. In the Crusader Kingdom in the mid-thirteenth century Philip of Novarra, a learned jurist, urged nobles to hire capable tutors for their sons; Riley-Smith, *Feudal Nobility and the Kingdom of Jerusalem*, 130. This was already the practice of the kings of Jerusalem; William of Tyre, for example, served as tutor to the young Baldwin IV; *ibid.*, 101.

⁷⁹ Letter to Theobald of Etampes, as cited in R. W. Southern, “Master Vacarius and the Beginning of an English Academic Tradition,” in J. J. G. Alexander and M. T. Gibson, eds., *Medieval Learning and Literature: Essays Presented to Richard William Hunt* (Oxford, 1976), 268, 268 n.

⁸⁰ Thorndike, “Elementary and Secondary Education in the Middle Ages,” 401–03; and Orme, *English Schools in the Middle Ages*, 66–67. For a description of a parish priest in the late eleventh century hurrying to administer the last rites *cum suis veniens scholaribus*, see Thomas Arnold, ed., *Memorials of St. Edmund’s Abbey*, 1, Rolls Series, no. 90 (London, 1890): 81. Mary Martin McLaughlin has suggested that noblewomen were capable of instructing their children in simple Latin; McLaughlin, “Survivors and Surrogates,” in Lloyd de Mause, ed., *History of Childhood* (New York, 1974), 125.

⁸¹ Knowles, *The Monastic Order in England*, 943–1216, 488–91; and Orme, *English Schools in the Middle Ages*, 226.

⁸² Johnson, *Dialogus de Scaccario*, xiv, 57; Warren, *Henry II*, 55–56; J. Stevenson, ed., *Chronicon Monasterii de Abingdon*, 2, Rolls Series, no. 2 (London, 1858): 229; and Orme, *English Schools in the Middle Ages*, 64–65.

Following the Conquest other types of schools gave organized classes in Latin grammar and more advanced subjects.⁸³ These schools can be grouped in four categories: (1) the “song schools” that instructed young choristers and outside pupils in elementary Latin at the nine secular cathedrals and at some collegiate churches; (2) grammar schools in London and a number of other important towns and at practically every English cathedral; (3) schools of *ars notaria* or what might be called “business schools” that taught the drafting of letters and legal documents, accounting, and some common law; and (4) “higher schools” at some cathedrals, at Northampton, and at Oxford that gave advanced instruction in the liberal arts, philosophy, and theology and sometimes in Roman and canon law comparable to that given later in the universities.⁸⁴ By the mid-twelfth century, these cathedral schools were accepting pupils who had no intention of progressing beyond minor orders into the priesthood.⁸⁵

If laymen in the king’s service earned any education beyond the elementary level, it was likely to have been at the “business schools.” Before the opening of these schools, any advanced training was purely practical, based on the students’ observations of experienced judges and other officials at work. Later, the Inns of Court attracted sons of noble families who sought to study the common law.⁸⁶ By the early thirteenth century a need for specialists with some academic training in the law had already appeared. As early as the reign of King John, Oxford seems to have become a center of business studies—*ars dictaminis*, accounting, and some elementary law—a curriculum that prepared youths for careers as common lawyers, estate stewards, conveyance clerks, or accountants. In the early thirteenth century several Englishmen composed treatises on the *ars dictaminis*, which are extant.⁸⁷ Another collection that has survived includes treatises on letter-writing, sample letters, and other materials, probably collected at Oxford as early as the first third of the thirteenth century.⁸⁸ These subjects were never part of the university curriculum but were studied in separate schools at Oxford into the fifteenth century.

Another question concerns the study of Roman law and its possible influence on the political thought of the men in the king’s service, for Roman law did arouse much interest in twelfth- and thirteenth-century England. Roman legal studies had their beginning in England during the reign of King Stephen

⁸³ For a list—albeit incomplete—of schools at nineteen different places in the years 1066–1149, see Orme, *English Schools in the Middle Ages*, 294.

⁸⁴ H. G. Richardson, “The Schools of Northampton in the Twelfth Century,” *EHR*, 56 (1941): 595–605, and “The Oxford School of Law under John,” 319–38; and Orme, *English Schools in the Middle Ages*, 64–65, 75–76, 80, 293–325.

⁸⁵ Bolgar, *The Classical Heritage and Its Beneficiaries*, 194.

⁸⁶ “Knights, barons and also other magnates and nobles of the realm place their sons in those inns, although they do not intend them to be imbued by a professional knowledge of the laws nor to live by its practice, but upon their patrimonies alone”; John Fortescue, *De Laudibus Legum Angliae*, ed. S. B. Chrimes (Cambridge, 1942), 118.

⁸⁷ Geoffrey de Vinsauf, John of Garland, Gervase of Melkley, and, somewhat earlier, Peter of Blois all composed treatises on the *ars dictaminis*. See Noël Denholm-Young, “The Cursus in England,” in F. M. Powicke, ed., *Oxford Essays in Medieval History Presented to Herbert Edward Salter* (Oxford, 1934), 75–80; and C. H. Haskins, “The Early *Artes Dictandi* in Italy,” in his *Studies in Medieval Culture* (Oxford, 1929), 191.

⁸⁸ H. G. Richardson, “An Oxford Teacher of the Fifteenth Century,” *Bulletin of the John Rylands Library*, 23 (1939): 447–49.

with the arrival of the Italian Master Vacarius. Although he established no school, there is some evidence for the study of Roman law at a number of centers by the mid-twelfth century.⁸⁹ Legal studies at these schools, however, cannot have attained a level comparable to that of the school that appeared at Oxford by the end of the twelfth century.⁹⁰ Generally, legal studies were on a lower level, using various compendia and condensations, not the *Digest* or the *Institutes* of Justinian. They grew out of a study of rhetoric, part of the *trivium*, for rhetorical studies had long included some legal teachings. The texts used in the medieval English schools must have resembled those of the early Italian law schools: the *ordo judicarius* and the formulary for the *ars notaria*.

The first, the *ordines judicarii*, were primarily manuals of procedure, attempting to remedy the deficiencies of contemporary canon and secular law by reference to Roman law. Sometimes they took on the characteristics of the formularies and vice versa.⁹¹ Several *ordines* written in either England or Normandy survive, the best known of which is by William Longchamp, Richard I's justiciar.⁹² Portions of another *ordo judicarius* survive as part of an Oxford compilation dating from the reign of King John. It may be derived from an earlier *ordo* studied in the twelfth-century schools at Northampton.⁹³

Another instrument for transmission of some Roman legal principles studied in the "business schools" was the notarial formulary. A course of study, the *ars notaria*, arose in medieval Italian municipal schools out of the *ars dictaminis*. It concentrated on the drafting of legal documents, and the formulary presented models for notaries public to follow. Portions of the Oxford compilation seem to represent an attempt by a master in early thirteenth-century Oxford to compile a formulary similar to those of Italy for use by English students preparing for careers in episcopal or noble secretariats. By the end of the thirteenth century, compilations of texts for study in the "business schools" included not only formularies for legal documents and manuals of procedure but also treatises on accounting and estate management.⁹⁴

The *ordines judicarii*, notarial formularies, and other texts indicate that it was possible for students who did not attend a university to gain some technical training for a business career. H. G. Richardson has described such students as young men "who were seeking employment in the households of the large landowners, where they would write their lord's letters, and keep

⁸⁹ For studies at Exeter, Hereford, Lincoln, Northampton, and St. Albans Abbey and possibly at London and York, see Stephan Kuttner and Eleanor Rathbone, "Anglo-Norman Canonists of the Twelfth Century," *Traditio*, 7 (1949-51): 321-23; and Kathleen Edwards, *The English Secular Cathedrals in the Middle Ages* (Manchester, 1949), 188-90.

⁹⁰ Kuttner and Rathbone, "Anglo-Norman Canonists of the Twelfth Century," 323. Also see Southern, "Master Vacarius and the Beginning of an English Academic Tradition," 257-86.

⁹¹ Richardson, "The Oxford School of Law under John," 336.

⁹² For the text of Longchamp's *Practica legum et decretorum*, see E. Caillemar, "Le droit civil dans les provinces anglo-normandes," *Mémoires de l'Académie de Caen* (Paris, 1883).

⁹³ Richardson, "An Oxford Teacher of the Fifteenth Century," 327, and "The Oxford School of Law under John," 319-38.

⁹⁴ Richardson, "The Oxford School of Law under John," 325, 328; and Dorothea Oschinsky, "Medieval Treatises on Estate Accounting," *Economic History Review*, 17 (1947): 57, and *Walter of Henley and Other Treatises on Estate Management*, 62, 234.

his courts and his accounts, and draw up for him conveyances and leases and other instruments as occasion arose.”⁹⁵ They might find a post with an abbot or bishop, with a sheriff or some other royal official, or perhaps with the king himself. Younger sons of knights who had to seek their fortunes or ambitious sons of humbler freemen very likely sought the training that would qualify them for such positions.

Once young men had completed their “business” course of six months to a year, they were ready to begin their careers, and any further training that they received came through everyday experience. Richard fitz Neal claimed that his treatise on the operations of the exchequer was more valuable than books of the philosophers, providing information that was *non subtilia sed utilia*.⁹⁶ A practical apprenticeship in some minor clerical post was preferred to a university degree as the proper preparation for a life of government service. Yet, obviously, an apprentice-clerk already had to have some knowledge of Latin letters, some primary education, before he could read Richard’s treatise.

TO ANSWER IN REVERSE ORDER THE THREE QUESTIONS raised earlier, it is clear that opportunities offered themselves for laymen to learn Latin grammar and to receive some specialized training in the twelfth and thirteenth centuries. As Lady Stenton wrote, “If men of position were illiterate, it was because they made no effort to learn rather than that provision for their teaching was not available.”⁹⁷ It is also clear, although the evidence is perhaps less illuminating, that more and more laymen were taking advantage of the available means of learning. Changes in government, requiring written records of all kinds, were making pragmatic literacy a necessity for the knightly class in the high Middle Ages. Finally, significant numbers of laymen in Angevin England did hold government posts that required literacy. That an adequate supply of literate knights existed to fill the new offices growing up in royal government is proven by the king’s ability to find laymen to fill them, laymen who performed tasks that certainly required that they know Latin.

⁹⁵ Richardson, “The Oxford School of Law under John,” 334.

⁹⁶ Johnson, *Dialogus de Scaccario*, 5.

⁹⁷ Doris M. Stenton, *English Society in the Early Middle Ages* (4th ed., Harmondsworth, Middlesex, 1971), 251.

The Military Class and the French Monarchy in the Late Middle Ages

JOHN BELL HENNEMAN

DURING THE PERIOD OF THE HUNDRED YEARS' WAR, European society exhibited both grave symptoms of decay and striking evidence of "renaissance," and historians of several nationalities have shown increased interest in this important time of transition. French political and institutional history affords a good example of the period's extremes of tragedy and triumph, for France experienced a particularly painful transition from "feudal monarchy" to "early modern state." The once prosperous kingdom of St. Louis encountered humiliating reversals under his Valois successors; yet out of this adversity emerged institutions that formed the basis of the French state until the end of the *ancien régime*. The century from 1345 to 1445 proved to be crucial to French history.

The disintegration and reconstruction of the French monarchy during this century were closely linked to two recurring problems: the financial position of the crown and the political stance of the nobility. Inadequate finances plagued the French during their early defeats, while the rapid development of financial institutions under Charles V (1364–80) coincided with a major French revival. Financial collapse after 1400 and the reconstruction of the fiscal system after 1435 accompanied a new cycle of defeat and victory. The weakness or alienation of important segments of the nobility also paralleled the periods of defeat, while the French monarchy triumphed in the 1370s and 1440s with the collaboration of this political and military elite. The essential ingredients of a strong royal army were adequate finances and effective support from the nobility. Thanks to recent scholarship on military organiza-

This is a much revised version of a paper originally presented at the Medieval Circle of the University of Virginia on April 21, 1976. The following abbreviations are used throughout in the notes: AD (Archives départementales); AM (Archives municipales); AN (Archives Nationales, Paris); BN (Bibliothèque Nationale, Paris); *Gr. Chartier* (*Archives de la Ville de Montpellier, Inventaires et documents: Inventaire du Grand Chartier redigé par Robert Louet*, ed. J. Berthelé, F. Castets, and O. de Dainville, 2 vols. [Montpellier, 1895–1955]); *HL* (C. De Vic and J. Vaissete, *Histoire générale de Languedoc*, ed. A. Molinier et al., 16 vols. [new ed., Toulouse, 1872–1904]); *INV* (*Inventaire sommaire des archives . . .*); NAF (Nouvelles acquisitions françaises); *Ord.* (D. F. Secousse et al., eds., *Ordonnances des roys de France de la troisième race recueillies par ordre chronologique*, 21 vols. [Paris, 1723–1849]); PO (Pièces originales); and *SPIC* (*Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions*).

tion and the development of taxation,¹ it is possible to summarize briefly the growth of the royal fiscal system and relate it to the political behavior of the nobles, particularly those who made a profession of service as fighting men and regularly commanded troops. It is this segment of French society that I shall call the "military class."²

THE FRENCH ARMY IN THE EARLY FOURTEENTH CENTURY still consisted mainly of people who owed customary service; an effective fighting force did not actually come into being until after the outbreak of hostilities. The government tried to finance wars by levying "subsidies" on those who did not render personal service, but the French expected their king to manage on the revenues of his own domains and accepted direct taxation only as a short-term emergency measure.³ Hoping to develop a more workable financial system, royal advisers invoked principles of Roman law. They claimed that the king, as guardian of the public welfare, could determine when necessity dictated extraordinary measures for the "common profit." In times of necessity he could override precedents or privileges to collect taxes, provided that he obtained, in some procedural way, the consent of those whose rights were affected.⁴ The king's subjects, however, were not prepared to accept his definition of a situation calling for taxes and long rejected all peacetime taxation. They usually defined "evident necessity" as a grave military threat to their own localities. The idea of consent, moreover, evoked little interest. Sometimes people resisted paying a tax that an assembly had granted; sometimes taxes were simply imposed and collected without discussion. The criterion was the public sense of emergency, and representative assemblies were more commonly expected to give counsel as to the need for taxes than to consent to specific subsidies.⁵

¹ The general reader will find an excellent recent treatment of fourteenth-century France in Barbara Tuchman, *A Distant Mirror: The Calamitous 14th Century* (New York, 1978). For the indispensable study of French military organization among scholarly works, see Philippe Contamine, *Guerre, état, et société à la fin du moyen âge* (Paris, 1971). For recent works on royal finance, see John Bell Henneman, *Royal Taxation in Fourteenth Century France: The Development of War Financing, 1322-1356* [hereafter *Royal Taxation, 1322-56*] (Princeton, 1971); John Bell Henneman, *Royal Taxation in Fourteenth Century France: The Captivity and Ransom of John II, 1356-1370* [hereafter *Royal Taxation, 1356-70*] (Philadelphia, 1976); Maurice Rey, *Le domaine du roi et les finances extraordinaires sous Charles IV, 1388-1413* (Paris, 1965); Maurice Rey, *Les finances royales sous Charles VI: Les causes du déficit, 1388-1413* (Paris, 1965); and Martin Wolfe, *The Fiscal System of Renaissance France* (New Haven, 1972).

² Since many nobles did not serve regularly as fighting men, the "military class" is not the exact equivalent of the "nobility." Leading royal commanders might be better described by such expressions as "military aristocracy" or "military elite," but these terms seem inappropriate for those who engaged in brigandage. The term "military class" permits me to speak of the fighting men without getting into questions of social origins that require additional research.

³ Henneman, *Royal Taxation, 1322-56*, esp. 3-11, 17-22, 27-32, 303-07. On the importance of obligatory service in this period, see Contamine, *Guerre, état, et société*, 536.

⁴ Gaines Post, *Studies in Medieval Legal Thought: Public Law and the State, 1100-1322* (Princeton, 1964), esp. 15-19, 310-22, 436-52. Also see the summary in Henneman, *Royal Taxation, 1322-56*, 22-25.

⁵ Henneman, *Royal Taxation, 1322-56*, 320-29. On the important distinction between counsel and consent, see Gavin Langmuir, "Counsel and Capetian Assemblies," *SPIC*, 18 (1958): 21-34.

These attitudes prevailed during the age of the war subsidy, the period up to 1356. The government generally limited the collection of taxes only to periods of outright war, with the result that France was left militarily unprepared when hostilities resumed after a truce expired. Although sometimes levied directly on personal wealth, subsidies usually were light indirect taxes on consumer goods.⁶ To tax the wealthy, the crown used expedients such as manipulation of the currency for profit; but, when the minted value of a mark of silver rose from three to fifteen *livres tournois* by 1343, the public outcry compelled revaluation. In the 1350s, the government again tried to augment inadequate revenues by altering the coinage, and the policy antagonized all social groups.⁷

The age of the war subsidy ended in 1356 when the defeat and capture of King John II (1350–64) led to decisive changes in French fiscal history. For a decade, central assemblies of the three estates had assumed increasing importance, and had they now capitalized on the prevailing sense of crisis to finance the king's release, the Estates might have made themselves indispensable. Instead, the assembly fell into discredit because the actions of ambitious individuals created divisions among classes.⁸ Two other consequences of the king's capture had even greater importance: the need to pay a large ransom, which necessitated the first regular peacetime taxation in French history; and the brigandage of soldiers left unemployed by the cessation of Anglo-French hostilities. To pay the royal ransom, the crown established in late 1360 indirect taxes known as the *gabelle* (on salt) and the *aides* (5 percent on the value of most other products, with a special rate for wine). Affecting primarily the urban population, these first peacetime taxes resembled earlier war subsidies but had higher rates. Intended to last for six years, they were actually collected in all but two of the next fifty-seven years, and their establishment permitted a reform of the currency that stabilized the minted value of the mark of silver at about six *livres tournois* for half a century.⁹

The brigandage that followed the king's capture had a profound effect on fiscal and constitutional developments in France. The Anglo-French war and peripheral conflicts in various border districts offered careers as soldiers of fortune to thousands of petty nobles who were uprooted from their home districts. They turned to plunder when their salaries were not paid, and the crown could not control them. The extent to which elements of the French nobility were counted among these *routiers*, as they were called, requires

⁶ For the early part of this period, see Joseph R. Strayer and Charles H. Taylor, *Studies in Early French Taxation* (Cambridge, Mass., 1939). For a brief summary of the taxes after 1322, see Henneman, *Royal Taxation, 1322–56*, 308–09, app. 2. Many so-called hearth taxes, notably those levied in southern France where they were apportioned among the communities, actually were raised by means of local levies on the sale of consumer goods.

⁷ See the tables in Harry A. Miskimin, *Money, Prices, and Foreign Exchange in Fourteenth-Century France* (New Haven, 1963), 161–90; and Henneman, *Royal Taxation, 1322–56*, 339–44.

⁸ For the most recent discussion of the Estates of 1356–58 and some of the bibliography on this subject, see Henneman, *Royal Taxation, 1356–70*, 20–83.

⁹ See the discussion in *ibid.*, 117–20. For the enacting ordinance, see *Ord.*, 3: 433–36.

¹⁰ See Siméon Luce, *Histoire de la Jacquerie* (2d ed., Paris, 1894), esp. 38 ff. and 50 ff.; and Contamine, *Guerre, état, et société*, 157, 543.

further study, but the populace blamed the nobility as a class for their behavior.¹⁰

The brigands forced local populations to look to their own defenses. Individual regions levied special taxes to hire soldiers or to buy off *routiers* who had captured local fortresses. Such initiatives gave importance to provincial estates at a time when central assemblies were becoming discredited. Localism of outlook, which had once hindered the development of royal taxation, now acquired a constructive character. People came to realize that "evident necessity" need not coincide with a formal state of war. Those who had resented light royal taxes in time of war now assessed themselves large sums, over and above the *aides*, to obtain some security in time of nominal peace. In the Norman district around Caen, for instance, the inhabitants paid three times as much in 1361, when England and France were at peace, as they had in wartime a few years earlier.¹¹ Auvergne was infested with *routiers* in the 1360s, and in frequent assemblies the representatives of the towns assessed taxes to finance countermeasures.¹² In Languedoc, where resistance to taxation in time of peace or truce had always been strong, the first tax to combat brigandage was levied, reluctantly, in 1358. Four years later, the towns of the Midi raised 153,000 florins in a vain attempt to bribe thousands of *routiers* into leaving for Spain. Taxes to deal with brigands continued to rise thereafter.¹³

France did not eliminate brigandage for eighty years, but finally did so using methods first devised in the 1360s. One technique was to send the *routiers* on a campaign outside the kingdom, but the most fruitful policy was to employ the more experienced and disciplined soldiers on a regular basis. The gradual adoption of this policy had a major impact, not only on royal finances and the royal army, but also on the political relations between the crown and the military class. The 1360s saw two major developments. The first came late in 1363, when Estates representing the greater part of the kingdom (Languedoc) met at Amiens and authorized a *fouage*, or apportioned hearth tax, averaging three francs per taxable household, to pay troops used to restore public order.¹⁴ The second occurred in Languedoc in 1368, where the king's brother, Louis of Anjou, was royal lieutenant. Mindful of earlier attempts to expel brigands from the kingdom, and eager to carve out a personal domain across the Rhone, Louis had Bertrand du Guesclin, the most prestigious captain of the period, lead an army of *routiers* to conquer Provence. These troops were paid

¹¹ On the *routiers* generally, see Edouard Perroy, *The Hundred Years War*, trans. W. B. Wells (London, 1951), 45, 154-57. For the main texts on the taxes in lower Normandy to combat brigandage, see BN, MS fr. 25700, nos. 87-89, 103; MS fr. 20581, no. 63; NAF 3654, nos. 37, 38; and NAF, 20026, nos. 36-39. Also see Alfred Coville, *Les états de Normandie, leurs origines et leur développement au XIV^e siècle* (Paris, 1894), 98-101, 374-75, 377-79; and Leopold Delisle, *Histoire du château et des sires de Saint-Sauveur-le-Vicomte* (Valognes, 1867), 118.

¹² *INV AM Riom*, CC 2; *INV AM Montferrand*, 1: 395-99 (CC 167-68); Marcellin Boudet, *Thomas de la Marche, bâtard de France, et ses aventures (1318-1361)* (Riom, 1900), *pièces justificatives*, nos. 46-48; and Françoise Lehoux, *Jean de France, duc de Berry: Sa vie, son action politique (1340-1416)*, 3 vols. (Paris, 1966-68), 1: 181-90.

¹³ *III.*, 9: 680, 747, 759, 766, 774-75, 10: cols. 1145-51, 1230, 1319-20, 1323-25; AD, Hérault A 5, ff. 141r-42; BN, Coll. Languedoc 85, f. 287r; *Le Petit Thalamus de Montpellier* (Montpellier, 1836), 360-70; Maurice Prou, *Relations politiques du Pape Urban V avec les Rois de France Jean II et Charles I^{er}* (Paris, 1888), 54-61, and *pièces justificatives*, nos. 44, 53, 57, 62, 63; and E. Molinier, *Études sur la vie d'Arnoul d'Andrehem, maréchal de France (1302-1370)* (Paris, 1883), 114-19, 134, 138, 140-41, 170-71.

¹⁴ *Ord.*, 3: 646; Adolphe Vuitry, *Études sur la régime financière de la France avant la Révolution de 1789*, new ser., 2 (Paris, 1883): 116-18.

by taxes levied on the towns of Languedoc, which had every reason to wish to see the soldiers employed outside their territory.¹⁵

The *fouage* of 1363, granted by a central assembly and collected without dispute or ratification by local assemblies, was the first direct tax to be levied throughout Languedoc in time of nominal peace. It affected the vast rural population as well as the towns. The *fouage* enabled the crown to employ some members of the military class and to punish others who continued to disturb the peace. Enacted without a stated time of expiration, the levy continued in force for seventeen years except for a brief suspension in 1369. It was the immediate ancestor of that major tax of the *ancien régime*—the apportioned *taille*.¹⁶ Louis of Anjou's maneuver of 1368, in some respects a southern counterpart of what had been done in 1363, sought more explicitly to employ the brigands themselves outside the kingdom's borders. The army thus created under Du Guesclin's leadership and later sent to Spain was then available for the renewal of the English war in 1369. In both parts of France, therefore, the violence of the *routiers* produced responses that led, almost inadvertently, to a kind of standing army and the taxes to support it. For the first time in forty years, France was prepared militarily for the resumption of hostilities and was able to win victories.

These developments had considerable effect on taxpayers, if the southern town of Montpellier was at all typical. The average annual burden per taxable hearth there was twenty-one times greater after 1368 than it had been in 1328.¹⁷ This revolutionary increase in the level of taxation resulted from the royal ransom and the struggle to suppress brigandage and thus ultimately from the capture of John II in 1356. Increased taxation of such magnitude was not easy to accept, particularly in times of recurrent plague and economic distress. A violent reaction against taxes occurred between 1379 and 1384. Beginning and ending in Languedoc, it engulfed the entire kingdom and was especially marked by the cancellation of both the *fouage* and the *aides* in the fall of 1380, when Charles V died and was succeeded by a minor. But the interruption was only temporary.¹⁸ The *aides* soon were back in force, and, while the royal government did not restore the *fouage* in Languedoc, it substituted periodic special taxes that began as surtaxes on the *aides* but soon took the form of apportioned direct taxes, henceforth called *tailles*.¹⁹

¹⁵ *Le Petit Thalamus*, 382; *HL*, 10: cols. 1396–99, 1415–16; *Gr. Chartrier*, nos. 3604, 3615, 3631, 3634, 3919–22; V. Bourilly, "Du Guesclin et le duc d'Anjou en Provence (1368)," *Revue historique*, 152 (1926): 162–64; and L. H. Labande, "Bertrand du Guesclin et les états pontificaux de France," *Mémoires de l'Académie de Vaucluse*, 4 (1904): 65–67.

¹⁶ Rey, *Le domaine du roi*, 165–67; Wolfe, *Fiscal System of Renaissance France*, 7, 305; and Leopold Delisle, ed., *Mandements et Actes Divers de Charles V (1364–1380)* (Paris, 1874), nos. 625, 637, 679.

¹⁷ These figures are developed at some length in Henneman, *Royal Taxation*, 1356–70, 259–65, and are based on *Gr. Chartrier*, nos. 475, 476, 3530, 3592, 3594–96, 3601, 3604, 3606, 3630–32, 3634, 3870, 3871, 3917–22, Dviii 77, Dviii 80. Comparisons are meaningful because the metallic value of the currency was about the same in 1328 as in 1368. The town's assessed number of taxable hearths, however, dropped from 6000 to 2300. For the continuing high level of taxation in Languedoc through the 1370s, see *HL*, 9: 821, 828–34, 841–42, 847, 852, 855, 862–67, 872–74.

¹⁸ For a summary of the reaction against taxes, with appropriate bibliography, see Henneman, *Royal Taxation*, 1356–70, 298–303.

¹⁹ *Ord.* 7: 746; and Rey, *Le domaine du roi*, 325–30.

In effect, the financial regime established in the 1360s survived for nearly half a century. The system's collapse was the belated result of the crisis occasioned by Charles VI's recurrent insanity after 1392. Rival princes appropriated royal revenues, and factional strife among them finally degenerated into civil war. The successful English invasion of 1415 and the ensuing treaty of 1420 left France effectively partitioned among the English, the duke of Burgundy, and the followers of the future Charles VII.²⁰ Royal finances completely collapsed in the years 1417–18, when the competing French factions cancelled the *aides* and resumed a massive weakening of the currency. For nearly twenty years thereafter, the French government functioned without the regular annual taxes that had so benefited Charles V, although assemblies of Estates did grant *tailles* in most years.²¹

The permanent re-establishment of regular taxation and a properly financed royal army finally came about between 1435 and 1445 in a series of developments that remarkably paralleled those of the 1360s. A treaty of peace (this time with Burgundy) was followed by a new imposition of the *aides* and *gabelle* and a stabilization of the currency.²² Again, many unemployed soldiers turned to brigandage, and the problem was confronted, as it had been in the 1360s, with makeshift measures by regional assemblies, a new direct tax to pay troops (granted by the Estates without a stated time of expiration), and the employment of *routiers* to undertake a dangerous foreign expedition.²³ The new royal direct tax was the *taille* of 1439, which bore considerable resemblance to the *fouage* of 1363–80 and the temporary *tailles* of the earlier fifteenth century. Together with the *aides* and *gabelle*, it restored the fiscal system of Charles V, and the continued levy of these taxes in subsequent generations enabled France to pay for the army and bureaucracy that an early modern state required.²⁴ The most important development of this period, however, was the military reform set in motion by the ordinances of 1445. Modeled on earlier experiments, this reform established the famous *compagnies d'ordonnance*, made up of salaried men-at-arms²⁵ and created as a direct response to the problem of brigandage. As in the 1360s, this force quickly showed its value when Anglo-French hostilities resumed. Between 1449 and 1453, the English were driven from France, and in the next generation the French monarchy broke the power of the princes and emerged as a great European power.

²⁰ See Perroy, *Hundred Years War*, 195–244.

²¹ Charles Petit-Dutaillis, vol. 4, pt. 2 of *Histoire de France*, ed. Ernest Lavisse (Paris, 1900–11): 28–35, 239–42; *ILL*, 10: cols. 1984–85; J. Lafaurie, *Les monnaies des rois de France de Hugues Capet à Louis XII*, 2 (Paris, 1936): 68–70, 95–102; and J. Russell Major, *Representative Institutions in Renaissance France, 1421–1559* (Madison, Wisc., 1960), 25–31.

²² Petit-Dutaillis, *Histoire de France*, 242–43; Perroy, *Hundred Years War*, 294–96; Wolfe, *Fiscal System of Renaissance France*, 29; and Lafaurie, *Les Monnaies des rois*, 108–11.

²³ A. Tuetey, *Les écorcheurs sous Charles VII*, 1 (Montbéliard, 1874): 36, 42–43, 123–244; E. Cosneau, *Le connétable de Richemont (Artur de Bretagne) (1393–1458)* (Paris, 1886), 261–65, 341–55, 566–67; Jules Quicherat, *Roderigue de Villandro* (Paris, 1879), 114–15, 121, 128, 131, and *pièces justificatives*, nos. 10, 37, 42, 43, 53, 58, 62, 63, 73; and Contamine, *Guerre, état, et société*, 249.

²⁴ Cosneau, *Le connétable de Richemont*, 297–98, 356; Contamine, *Guerre, état, et société*, 142–44, 278; Wolfe, *Fiscal System of Renaissance France*, 34–35; and Petit-Dutaillis, *Histoire de France*, 243–45.

²⁵ Cosneau, *Le connétable de Richemont*, 356–57, 614–16; and Contamine, *Guerre, état, et société*, 277–319.

TABLE I
One Hundred and Eighty Prominent Military Commanders, 1360–1407

<i>Northwestern France</i>			<i>Central and Southeastern France</i>		
<i>Region</i>	<i>Number</i>	<i>Percentage</i>	<i>Region</i>	<i>Number</i>	<i>Percentage</i>
Normandy	66	36.7%	Ile-de-France	14	7.8%
North	25	13.9%	Center	13	7.2%
West	22	12.2%	East	12	6.7%
Brittany	21	11.7%	South	7	3.9%
	134	74.5%		46	25.6%

NOTE: For the criteria used in selecting these 180 men, see note 28, below.

THE ESTABLISHMENT OF REGULAR ROYAL TAXATION in the 1360s and the permanent re-establishment of that system after 1435 clearly made possible what has been called the “standing army,” but the traditional view of cause and effect must now be modified. The taxes were used to pay troops, but the behavior of unpaid troops was what forced people to accept the fiscal burden. Thus, the military element in French society had effectively compelled the kingdom to provide support for a military establishment. A complete understanding of this phenomenon must await further research,²⁶ particularly a study of those who engaged in brigandage. Philippe Contamine’s massive work on war and society in late medieval France did not address the problem squarely. To do so, one must compare the principal captains of *routiers* with the principal commanders of royal troops to determine what similarities and differences existed between the two groups.

The half-century that began in the late 1350s provides fertile ground for such a project because it was the first period to endure serious brigandage and because the procedures for mustering troops in those years left documents that identify the leading royal commanders. In contrast to most of the captains of *routiers*, the royal commanders—the elite of the “military class”—can generally be traced geographically and genealogically. Preliminary research has yielded information about their political behavior and that of their families from the early fourteenth century onwards—information that demands close attention. The extensive military pay records of the later fourteenth century contain the names of more than sixteen-hundred men who received salaries for commanding royal forces, and further research will doubtless uncover more.²⁷ Less than three hundred and fifty of them, however,

²⁶ Raymond Cazelles proposes to entitle his forthcoming work on the later fourteenth century “La revanche de la noblesse.”

²⁷ In addition to the more than 3050 volumes of *Pièces Originales*, for the main sources of unpublished information about military commanders, see BN, Clairambault 1–203; BN, MS fr. 7858, 21495, 21539, 25764–66, 32510, 32511; BN, NAF 7414, 8603, 8604, 20528, 23634; AN, K 53, 54; AN, AB xix 690–93. Hundreds of other relevant documents are scattered through other collections. For the most valuable published materials, see G. A. La Roque de la Lontière, *Histoire généalogique de la maison de Harcourt* [hereafter *Histoire*

appear in the documents more than a few times, and a still smaller number exercised command with sufficient frequency to qualify as "prominent commanders." Preliminary analysis has produced a list of one hundred and eighty French nobles whose places of origin can be established with reasonable certainty and whose frequent appearances in the military records indicate that they belonged to the elite of the French "officer corps," the true military aristocracy of the later fourteenth century.²⁸

In terms of their geographical origins, these military leaders were distributed very unevenly over the map of France. For purposes of analysis, the kingdom may be divided into eight large regions: the *North* (Picardy, Artois, Vermandois, and Beauvaisis); the *East* (Burgundy and Champagne); the *Center* (Berry, Bourbonnais, and Auvergne); the *South* (the *langue d'oc* south of the Dordogne river and the Massif Central, including Valentinois and Dauphiné); the *Ile-de-France* (the viscounty of Paris and the bailiwicks of Senlis, Chartres, and Orleans); *Normandy* (including the counties of Alençon, Perche, and Eu); the duchy of *Brittany*; and the *West* (Anjou, Maine, Touraine, Poitou, Limousin, and Saintonge). The origins of the 180 prominent commanders have been tabulated in Table 1. This tabulation slightly underestimates the importance of Brittany because several commanders from Anjou and Maine (members of the Craon and Laval families, for instance) had lands in Brittany, figured prominently in Breton politics, and could easily be regarded as Breton even though they are assigned to the West.²⁹ Changes in the size of the sample, however, do not alter significantly the geographical distribution, and it remains incontestable that close to three-quarters of the men who led the royal armies in the decades after 1360 originated from a broad belt of lands extending counterclockwise from the Flemish frontier in the north to Saintonge and the Dordogne in the west. The nobility of the north and west

de Harcourt], 4 vols. (Paris, 1662); P. Hay du Chastelet, *Histoire de Bertrand du Guesclin, connestable de France* (Paris, 1666); Henri Moranvillé, *Étude sur la vie de Jean le Mercier* (Paris, 1888); V. Hunger, *Quelques Actes normands des XII^e, XIV^e, et XVI^e siècles*, 3 vols. (Paris, 1909-11); Père Anselme de Sainte-Marie, *Histoire généalogique et chronologique de la maison de France*, 9 vols. (3d ed.; Paris, 1726-33); La Chesnay des Bois, *Dictionnaire de la Noblesse*, 12 vols. (Paris, 1780); Gustave Dupont-Ferrier, *Gallia Regia*, 7 vols. (Paris, 1942-65); and H. Morice, *Mémoires pour servir de preuves à l'histoire civile et ecclésiastique de Bretagne*, 3 vols. (Paris, 1742-46). For other sources and bibliography, see Contamine, *Guerre, état, et société*.

²⁸ In listing forty leading captains for the period 1369-80, Contamine used the criterion that they were retained by Charles V for at least two complete years or in four different years: *Guerre, état, et société*, 562-93. My list of prominent commanders is based on the much longer period, 1360-1407, and on two criteria that seem important: (1) the number of years in which a person exercised command; and (2) the number of documents in which he is mentioned. For a given individual, the sum of these two figures can be called his "prominence factor." Some 94 persons had totals of 40 or more and another 101 had totals of 24-39. Only a few had "prominence factors" in the 21-23 range; more than 90 fell in the 15-20 range; and hundreds of others had less than ten. Those with totals of less than 24 tended to be much more obscure, and many have been impossible to trace genealogically or geographically. In general, therefore, I have described as "prominent" those with a total of 24 or more, adding to the list those who were marshal or constable of France in this period and subtracting those who were foreigners, or royal princes, or who served in less than six different years. I have also excluded those few whose geographical origin I have not yet determined. Where Contamine was concerned with the structure and organization of the army over two centuries, my purpose has been merely to identify the leaders of that army between 1360 and 1407.

²⁹ Four men who were prominent in Breton politics—Guy XII de Laval, Brumor de Laval, Jean de Beaumanoir, and Pierre de Craon—have been assigned to the West because their principal lordships lay in Anjou or Maine. On the other hand, several members of the Mauny family, who had lands and influence in Normandy, were called Bretons in contemporary documents.

(broadly conceived to include Normandy and Brittany) dominated the royal military leadership during a period when France was, on the whole, militarily successful.³⁰

This geographical distribution is highly significant because of the political history of the nobility in the first half of the fourteenth century. In the generation of French defeats that preceded 1360, the nobility of the north and west led the opposition to the Valois monarchy, as the studies of Raymond Cazelles have shown. Robert of Artois, disgraced by Philip VI (1328–50), had many friends and followers in the north. So also did Raoul d'Eu, whom John II executed early in his reign.³¹ The Evreux branch of the royal house, based in Normandy and led by Charles "the Bad" (king of Navarre, 1349–87), was a major focus of opposition to the crown and had the support of many nobles of the north and west.³² So also did the Montfort branch of the ducal house of Brittany, which England supported against the French-backed claimant to the duchy. The closely interrelated aristocracy of these regions was under-represented in the government of Philip VI, who relied mainly on advisers from the Center and East.³³ The political alignment of the northwestern nobles thus underwent a dramatic change: in the 1340s and 1350s, they were hostile to the crown; in the 1370s and 1380s, they dominated the leadership of the royal army. Did a royalist party merely gain the upper hand over some previously dominant anti-Valois faction? Or did some influential people actually change sides, and, if so, why?

Prominent among the earlier opponents of the crown was the Harcourt family of Normandy. Among those identified with the Harcourts or temporarily disgraced for opposing Philip VI were Jean Tesson, Guillaume de Briqueville, and Jean Tournebu. The Angevin lord of Craon, who had served the English king, was not trusted by the French, and his cousin, Olivier III de Clisson, was executed for treason in 1343. Not only did the Harcourts have marriage connections with the Briqueville, Craon, Tesson, and Tournebu families, but the Harcourts also were related to such other families as the Braquemont, Boissay, Estouteville, Ferté-Fresnel, Harenvilliers, Ivry, Meulan, Paynel, Saquainville, and Villiers, all of whom are found on the list of prominent commanders of the next generation.³⁴ Other families on the list

³⁰ The words North and West are capitalized in this article only when referring to the specific regions among the eight just described. In saying "the nobility of the north and west," I am referring to the four regions, including Normandy and Brittany, that collectively made up northwestern France. If we consider only those commanders with "prominence factors" of 40 or more, we find that 71 percent were from the four regions of the north and west. Of those with a total of 24–39, 78 percent were from these regions. Of the next 120 commanders in frequency of service, the origins of 48 have not been determined, but 53 of the remaining 72 are from the north and west. Philippe Contamine has observed that Brittany, Anjou, and Maine had a high density of nobles, while Poitou, Saintonge, and Limousin had an average density, and the lands we have called here the Center and East had a low density; "The French Nobility and the War," in Kenneth Fowler, ed., *The Hundred Years War* (London, 1971), 139. If this pattern helps explain the distribution of our military commanders, it also explains the importance of the nobility of the north and west.

³¹ Raymond Cazelles, *La société politique et la crise de la royauté sous Philippe de Valois* (Paris, 1958), 81–84, 135–39.

³² Raymond Cazelles, "Le parti navarrais jusqu'à la mort d'Etienne Marcel," *Bulletin philologique et historique* (1960), 839–69.

³³ Cazelles, *La société politique et la crise*, 133, 272.

³⁴ *Ibid.*, 136–54. Also see the many notices on family connections found throughout La Roque, *Histoire de Harcourt*.

had been associated with Charles of Navarre and the Evreux family in opposing the king during the 1350s or earlier: the Clères, Coucy, Fiennes, Garencières, Hangest, Martel, Le Masuyer, and Raineval, along with some of those already mentioned as connections of the Harcourts.³⁵ This enumeration of families is very superficial and it neglects a multitude of other marriage connections among our "prominent commanders" that have not yet been analyzed carefully. Yet the twenty-three families just mentioned provided more than one-quarter of the northwestern nobles who dominated the royal military leadership after 1360. Even an incomplete genealogical analysis indicates that many prominent families, who had supported the opposition in earlier decades, changed sides by the reign of Charles V. What caused the political shift that now aligned this regional aristocracy with the crown?

Relations between the royal government and the nobility as a whole, as well as particular circumstances in northwestern France, contributed to the rapprochement. During the age of the war subsidy, three issues particularly affected the relations between crown and aristocracy: (1) the level and regularity of military salaries; (2) the royal manipulation of the coinage; and (3) a somewhat ill-defined clamor for "reform" that embodied strong criticisms of the way in which royal finances were managed and a desire for more regional autonomy.³⁶ Attempts to grapple with these issues at the general Estates had been frustrated, first by the Black Death of 1348 and then by the defeat and capture of the king at Poitiers. During the king's captivity, the Estates came under the domination of men who alienated much of the nobility by turning the reform movement into a vendetta against personal rivals and by demanding sharply higher taxes on noble fortunes. The murder of two important military commanders by a Parisian mob early in 1358 created a decisive cleavage between the nobility and the bourgeois leaders of the Estates, which was aggravated four months later by the brief but savage peasant uprising known as the *Jacquerie*.³⁷ When the future Charles V, acting as regent for his captive father, entered Paris triumphantly in August, the royal house enjoyed a degree of good will extended by the nobles that it had not experienced for decades.

Charles had done nothing to earn this support; the nobles rallied to the crown because of their repugnance for other groups associated with the opposition. Perhaps they were also moved by honor and sentiment: John II, who had refused to flee from the battlefield, commanded greater respect after his unfortunate capture than before it. Motives such as these hardly ensured a permanent rapprochement, and many nobles might well have drifted back into opposition had not the crown adopted conscious policies to retain their support. Although John II has been stereotyped as a foolishly chivalric figure whose sympathies lay with the nobles, Raymond Cazelles has pointed out that the king actually employed unpopular advisers of lowly origin while alienating important segments of the nobility. Conversely, Charles V, whom

³⁵ Cazelles, "Le parti navarrais," 845-65.

³⁶ *Ibid.*, 850-52. Also see the comments in Henneman, *Royal Taxation*, 1356-70, 19-21.

³⁷ On the Estates and the alienation of nobles during the *Jacquerie*, see Henneman, *Royal Taxation*, 1356-70, 14-18, 25-78. The basic work on the uprising itself remains Luce, *Histoire de la Jacquerie*.

Cazelles does not admire, made substantial concessions to the military class, despite his traditional image as an unchivalric pragmatist.³⁸ This deference to the aristocracy did not make Charles an ineffective ruler; indeed, it ensured the success of the monarchy after the 1360s. For, notwithstanding the myth that "antifeudal" bourgeois advisers somehow made for better government, the most successful monarchs always were those who knew how to cultivate the nobles and direct their aspirations toward royal ends.

In the years around 1360, the government moved to solidify the support of the nobility by appropriating from the former opposition some of the more popular reforms and reformers, and by reckoning military salaries in gold coin rather than in the unstable money of account.³⁹ Above all, however, the nobles benefited from the ordinance of late 1360 that established the *aides* and *gabelle*. Not only did these taxes bear mainly on the urban population rather than on the nobility, but the ordinance stabilized the coinage. Finally, the *fouage* imposed in late 1363, which did affect the nobles and their subjects, was levied to pay military salaries and provide legitimate employment for nobles who were widely blamed for brigandage. Charles V, unusually successful in paying his troops regularly, thereby further improved his standing with the military class.⁴⁰

In addition to these general developments, events of local significance in the north and west produced a realignment of the nobility there. Initially, opposition to the crown in the northwest had two focal points: in Normandy, where the Evreux family led by Charles of Navarre wielded considerable power, and in Brittany, where the ducal succession was still disputed. In these regions, two pitched battles in 1364 radically altered the political situation. In May, a royal army crushed the forces of Charles of Navarre at Cocherel.⁴¹ Once a formidable leader of disaffected nobles, this prince had lost many supporters through his association with the Parisian leaders in 1358, his evident lack of enthusiasm for the release of John II, and his subsequent involvement with notorious brigand chiefs. His military defeat cost him lands, prestige, and political influence. Later in 1364, at Auray in Brittany, a second battle ended the critical phase of the dispute over the Breton succession. The English-backed John of Montfort won decisively and Charles of Blois, leader of the Penthhièvre faction, was killed. The defeat was a reversal for the French, who now had to accept John IV as duke,⁴² but it created conditions in the duchy from which the king of France would profit.

³⁸ Raymond Cazelles, "Jean II le Bon: Quel homme? Quel roi?" *Revue historique*, 232 (1974): 5–26.

³⁹ Contamine, *Guerre, état, et société*, 96–98, 116–17. On the problems arising from coinage fluctuations, see the appendixes in Henneman, *Royal Taxation, 1322–56*, 331–53, and *Royal Taxation, 1356–70*, 312–16. On the shift of some reformers to the royal camp, see Cazelles, "Le parti navarrais," 868.

⁴⁰ Regarding military salaries, see Contamine, *Guerre, état, et société*, 146, 163–65. A recent work concerned with eastern Normandy argues that the earlier resistance to the crown was caused by economic troubles that made the lords resent taxation, whereas economic revival in the last third of the century made taxation more bearable and reduced opposition. See Guy Bois, *Crise du féodalisme* (Paris, 1976), 256–82.

⁴¹ Siméon Luce, *Histoire de Bertrand du Guesclin et de son époque* (Paris, 1876), 439–40; and Roland Delachenal, ed., *Chroniques des règnes de Jean II et de Charles V*, 2 (Paris, 1910): 1–2.

⁴² Delachenal, *Chroniques des règnes*, 5–8; and M. Jones, *Ducal Brittany, 1364–1399* (Oxford, 1970), 1–2.

Once recognized as duke of Brittany, John IV no longer depended on English aid and could try to steer a middle course between the two monarchies. This policy effectively deprived the English of their Breton base of operations. At home, John IV had to confront those Bretons whose lands had been acquired by English soldiers of fortune. He also found that his efforts to rebuild ducal power created resentment among lords who had faced few restraints during the long civil war. Those with lands and connections outside the duchy looked for support to their friends and relations to the east, and ultimately to the king of France.⁴³ Among the chief royal captains of the 1370s, Contamine found Bretons to be “the most coherent group.”⁴⁴ Although the influx of Breton nobles into royal service may be partly attributable to Bertrand du Guesclin’s appointment as constable of France, this explanation does not, by itself, appear sufficient. It is likely that a stronger influence on the Breton aristocracy was exerted by someone else, a prominent lord who changed sides in 1370 and soon became a pivotal figure in French politics: Olivier IV, lord of Clisson.

Olivier de Clisson had been a child at the time of his father’s execution for treason, but he naturally had grown up supporting the Montfort faction in Brittany and he had fought against Du Guesclin and the pro-French party at Auray. He had, however, become disgruntled when Duke John tried to prevent the Breton lords from taxing their seigneurial subjects, while Charles V, by contrast, remitted the *fouage* on his Norman lands for two years. In 1370, Clisson was ready to shift his allegiance, and in an exchange with the count of Alençon he traded some of his Norman lands for Josselin castle in Brittany, which he then agreed to hold as a vassal of the king of France rather than of the Breton duke. This action precipitated a feud with John IV that lasted twenty-five years.⁴⁵ Olivier then established relations with the Penthhièvre party—Charles of Blois’s daughter, the duchess of Anjou, and his son, John, who was still a captive in England.⁴⁶ In October 1370, Clisson made an alliance with Du Guesclin, and in 1371 he became Charles V’s lieutenant in Poitou. Henceforth, he regularly commanded large numbers of troops for the king of France, and many important Breton lords joined him.⁴⁷

Clisson’s case is the best documented, but other influential figures like the count of Harcourt and the lord of Coucy also became aligned with the crown at about the same time. It seems possible to conclude that a radical realignment of the northwestern nobility occurred because royal policies benefited nobles generally, political and military events in Normandy and Brittany favored the crown, and certain personalities influenced other lords to support the king. These developments occurred between 1358 and 1370, precisely the

⁴³ Jones, *Ducal Brittany*, 48–54.

⁴⁴ Contamine, *Guerre, état, et société*, 152.

⁴⁵ Jones, *Ducal Brittany*, 54 n. 5; Morice, *Mémoires pour servir de preuves*, 1: cols. 1639–40; and A. Lefranc, *Olivier de Clisson, connétable de France* (Paris, 1888), 84–85.

⁴⁶ See Jones, *Ducal Brittany*, 54, 94–101; and Lehoux, *Jean de France*, 2: 19.

⁴⁷ Morice, *Mémoires pour servir de preuves*, 1: cols. 1642, 1666. For examples of large contingents under Clisson’s command that included numerous Breton lords, see *ibid.*, 2: cols. 202–07.

period in which the monarchy achieved its major financial successes. Together, the political support of the nobility and the newly developed royal fiscal structure were largely responsible for French successes in the war of 1369–89 and for the favorable reputation that posterity has bestowed on Charles V.

THE NEW ALIGNMENT AND SUBSEQUENT POLITICAL BEHAVIOR of the nobles in the northwestern regions had considerable importance in the political turmoil that followed the death of Charles V in 1380. They tended to support policies associated with the late king and to resist the growing influence of the dukes of Burgundy. The fortunes of the monarchy in the next sixty years closely paralleled the fortunes of the northwestern military elite. Always unhealthy, Charles V in 1374 issued an ordinance in anticipation of a royal minority, naming his oldest brother, Louis of Anjou, as prospective regent and providing lesser roles for his younger brothers, John of Berry and Philip of Burgundy.⁴⁸ Soon after the king's death, however, Philip of Burgundy engineered the overthrow of this arrangement, and had the eleven-year-old Charles VI crowned and declared a major. The power of the French government now resided jointly in the royal uncles. The middle brother, John of Berry, was an incompetent administrator with expensive tastes. His great ambition, apparently, was to become royal lieutenant in Languedoc and enrich himself at the expense of the taxpayers of the Midi. Louis of Anjou and Philip of Burgundy were much more able and both had foreign ambitions, in Italy and the Low Countries respectively. Philip was in much the stronger position, not only because he had married the heiress to Flanders, but because the Netherlands were near enough to Paris to enable him to exert continuing influence on the French government without neglecting his foreign interests. Anjou, by contrast, left France in 1382 to seek the kingdom of Naples, and died in debt two years later.⁴⁹

Since Louis of Anjou had married Marie of Blois, daughter of the unsuccessful claimant to Brittany, he was on friendly terms with opponents of Duke John IV, such as Du Guesclin and Clisson. By contrast, Philip of Burgundy's wife was closely related to John IV and disliked his enemies. Differences between the royal brothers, in their political allies as well as in matters of policy, were the first signs of a new factionalism in French politics that deeply involved the northwestern nobility.⁵⁰ Du Guesclin died in 1380, and in the fall of that year the uncles debated the question of naming a successor. They finally settled on Clisson, who was not Philip of Burgundy's first choice and

⁴⁸ *Ord.*, 6: 26–32.

⁴⁹ See the detailed treatment in Lehoux, *Jean de France*, 2: 13–61.

⁵⁰ Because of his early departure, Anjou's personal role would be minimal, but many of his policies and alignments would be taken up in the 1390s by his nephew, another Louis, who was Charles VI's younger brother. This prince also became the rival of the duke of Burgundy and supporter of French intervention in Italy, and like his uncle of Anjou, he was friendly with Olivier de Clisson; Jacques d'Avout, *La querelle des Armagnacs et des Bourguignons* [hereafter *Armagnacs et Bourguignons*] (Paris, 1943), 25; Lefranc, *Olivier de Clisson*, 312, 329; and Lehoux, *Jean de France*, 2: 198.

whose appointment Philip later regretted. Clisson quickly became the leader of a party known as the “Marmousets”—former advisers of Charles V who wished to continue the late king’s policies, and whose sympathies lay more with Louis of Anjou than with the other royal uncles.⁵¹

By the end of 1382, Philip of Burgundy was able to dominate the French government, maintaining a correct, if uneasy, relationship with the Marmousets. The French position in the Low Countries and Brittany became stronger, but when plans to invade England were not carried through, the war turned into something of a stalemate. Then, in 1387, a crisis that centered on Olivier de Clisson rocked the government. The constable, who was organizing the latest French project for invading England, was arrested by his enemy, the duke, while he was in Brittany. John IV was dissuaded from putting him to death, but he extorted the promise of a heavy ransom and the surrender of numerous fortresses.⁵² This misfortune provoked a gleeful reaction from the dukes of Berry and Burgundy, who disliked Clisson,⁵³ but a number of prominent military commanders supported the constable, as did the nineteen-year-old Charles VI, who was devoted to him.⁵⁴ In general, the French nobility reacted with indignation against Brittany, and Clisson’s supporters soon recaptured most of the surrendered castles. The duke had to submit to a royal judgment that favored Clisson, and the French now ransomed from England John of Penthievre, the rival claimant to Brittany, who married Clisson’s daughter soon after his release from captivity.⁵⁵

In short, the constable and the Marmousets turned the whole incident into a triumph at the expense of the royal uncles, and in the fall of 1388 Charles V’s brothers were forced from power: Clisson boasted that he had made the king “seigneur of his own kingdom.”⁵⁶ Ruling with the advice of the Marmousets, Charles VI instituted a series of reforms aimed at restoring the more frugal government of Charles V. The government concluded a truce with England and replaced Berry’s corrupt regime in Languedoc with a team of royal *réformateurs*. The crown made improvements in the fiscal machinery and took concerted measures against brigandage, but did not impose a new *taille*.⁵⁷

⁵¹ Lefranc, *Olivier de Clisson*, 217–19, 267 ff.; Lehoux, *Jean de France*, 2: 14–15; Morice, *Mémoires pour servir de preuves*, 2: cols. 296–97; Moranvillé, *Jean le Mercier*, 83; and J. J. N. Palmer, *England, France, and Christendom*, 1377–1399 (Chapel Hill, 1972), 19.

⁵² Morice, *Mémoires pour servir de preuves*, 2: cols. 540–42; Moranvillé, *Jean Le Mercier*, 112–13; and Lehoux, *Jean de France*, 2: 205. For some general remarks about the military stalemate, see Palmer, *England, France, and Christendom*, 1–3.

⁵³ Lehoux, *Jean de France*, 2: 211–12. Berry and Burgundy actually had made a secret treaty with the duke of Brittany that probably was aimed at Clisson. See Morice, *Mémoires pour servir de preuves*, 2: col. 534; and BN, NAF 7620, f. 110.

⁵⁴ Lefranc, *Olivier de Clisson*, 311–13; Moranvillé, *Jean le Mercier*, 112–13; and Lehoux, *Jean de France*, 2: 211–12. Three particularly prestigious captains consoled and supported Clisson: Enguerrand de Coucy, Waleran de Luxembourg, and Jean de Vienne (admiral of France).

⁵⁵ Palmer, *England, France, and Christendom*, 101; Jones, *Ducal Brittany*, 106; Morice, *Mémoires pour servir de preuves*, 2: cols. 528–29, 543–47, 552–55; BN, NAF 7620, ff. 177r–90v; and Lehoux, *Jean de France*, 2: 216–23.

⁵⁶ Richard Vaughan, *Philip the Bold: The Formation of the Burgundian State* (Cambridge, Mass., 1962), 42; Lehoux, *Jean de France*, 2: 229–30; Moranvillé, *Jean le Mercier*, 118–19; and Lefranc, *Olivier de Clisson*, 327.

⁵⁷ Rey, *Le domaine du roi*, 63, 100–03; Rey, *Les finances royales sous Charles VI*, 571–72; Lehoux, *Jean de France*, 2: 177–79, 246–59; Moranvillé, *Jean le Mercier*, 122–25, 134–36; M. Nordberg, *Les ducs et la royauté: Études sur la rivalité des ducs d’Orléans et de Bourgogne, 1392–1407* (Uppsala, 1964), 40.

The overthrow of the royal uncles was a remarkable political development. Philip of Burgundy, an unusually able royal prince with great power at the royal court, had been ousted by an opposing faction, whose most visible members were seigneurs of middling rank or less and whose leader, the constable, had suffered humiliation and had narrowly escaped death just sixteen months before. From what source did the Marmousets derive the power required for such a coup d'état? There seems little doubt that their main support came from the prominent military commanders. At the time of the coup several of them were on the royal council: Mouton de Blainville, Louis de Sancerre, Jean de la Personne, Olivier de Clisson, and Bureau de la Rivière. They soon were joined by Pierre de Villaines, Guillaume des Bordes, Jeannet d'Estouteville, and Guillaume de Melun. Together with several bishops and three other lords whose role was more financial than military, these commanders formed the solid working majority of the council, and they began to develop a close relationship with the king's trusted younger brother, Louis of Touraine, later duke of Orléans, who was fond of Clisson.⁵⁸ The constable remained the key figure because of his prestige and influence among the crucially important nobility of the north and west. Cultivated by Charles V and drawn into the royal camp, these nobles had reason to support the continuation of Charles's policies. Most of them had served with Clisson, were related to his family, or shared his outlook and interests. Perhaps also, the ascendancy of Burgundy reminded them of the reign of Philip VI, when their region had felt slighted by a king who relied heavily on Burgundians.

During the years of their ascendancy, the Marmousets became increasingly associated with the king's brother, who became duke of Orléans in 1392.⁵⁹ As early as 1389, Louis counted among his chamberlains two important commanders from the northwestern nobility, Jean de Garencières and Pierre de Craon (Clisson's second cousin), as well as Jean le Mercier, the principal financial officer in the Marmouset party.⁶⁰ Other important military leaders, particularly from the north and west, appeared on the young duke's payroll during the 1390s.⁶¹ Despite his political leanings and his high princely rank, however, the pleasure-seeking Louis of Orléans was not yet mature enough to assume a role of leadership when the government was convulsed by a new crisis, precipitated by his own favorite, Pierre de Craon.

A man with an unsavory past, already embroiled in a dispute with the duchess of Anjou, Craon compounded his difficulties when he indiscreetly told the duchess of Orléans about one of her husband's escapades. He was expelled from court late in 1391 and took refuge with the duke of Brittany.⁶²

⁵⁸ Lefranc, *Olivier de Clisson*, 331–33.

⁵⁹ Avout, *Armagnacs et Bourguignons*, 15; and E. Jarry, *La vie politique de Louis de France, du d'Orléans 1372–1407* (Paris, 1889), 89–92. On Louis's new apanage, see also *Ord.*, 7: 467–72; AN, PP 109, p. 577; AN, K 54, no. 15.

⁶⁰ BN, PO 922, doss. 20385, nos. 19, 20, 32; PO 1280, doss. 28818, nos. 51, 52; and Moranvillé, *Jean le Mercier*, 136, 362–66.

⁶¹ BN, MS fr. 26028, no. 2408; MS fr. 26029, no. 2638; PO 61, doss. 1340, no. 16; PO 494, doss. 11108, no. 16; PO 875, doss. 19660, nos. 26, 27; PO 2989, doss. 66404, nos. 4–7; and PO 2883, doss. 64022, no. 52.

⁶² Robert Charles, *Histoire de la Ferté-Bernard* (Paris, 1876), 67–68; Moranvillé, *Jean le Mercier*, 141; Lefranc, *Olivier de Clisson*, 346–48; and Jones, *Ducal Brittany*, 129 n. 1.

Perhaps persuaded by John IV, Craon seems to have concluded that his powerful cousin, Olivier de Clisson, was responsible for his misfortunes, and on the night of June 13, 1392 he tried to assassinate the constable. Whatever the full truth of the matter may have been, the French government immediately blamed the duke of Brittany. Although he was then convalescing from a serious illness, Charles VI rose to Clisson's support with the same enthusiasm he had shown five years before, compelling his reluctant uncles to join him on a punitive expedition against Brittany.⁶³ The king's ill-considered decision to risk his health in this endeavor proved catastrophic for his realm. As the army proceeded westward from Le Mans in August, Charles suffered a serious attack of insanity. Not until October was he himself again, and a relapse occurred in the spring of 1393. Thereafter, the periodic "absences" of the king became the dominant problem of French politics.⁶⁴

The immediate consequence of the king's first attack of insanity was the overthrow of the Marmousets by the dukes of Berry and Burgundy. Clisson, Rivière, and Le Mercier were dismissed from office and played only a minimal role in royal service thereafter. Others, such as Pierre de Villaines and Jeannet d'Estouteville, lost power for a time but later reappeared on the council.⁶⁵ The uncles avoided taking violent revenge on their opponents, probably out of political shrewdness.⁶⁶ They had no way of knowing when, or whether, the king would recover, and it was not prudent to make martyrs of nobles whose party had been strong enough to seize power from the dukes just four years earlier. In the winter of 1392-93, during Charles VI's first recovery of lucidity, they had to acquiesce in an ordinance that made Louis of Orléans regent if Charles died leaving a minor heir.⁶⁷

The uncles were able to resume power because the party that had backed the Marmousets in 1388 had been thrown into disarray by the events of 1392. Early in the year, the crown and the duke of Brittany had reached an apparent accord,⁶⁸ and it is likely that many important military commanders doubted that John IV was really behind Craon's attack on Clisson. The attack itself was something of a family affair, and it must have left many with divided loyalties, since the Craon family was influential among the nobles of the north and west. The army of 1392 contained fewer of the prominent commanders and numerous small contingents headed by simple squires who had not held many commands previously. The important Breton group had a sharply reduced role in the royal army from this time onwards.⁶⁹ Louis of Orléans,

⁶³ Lehoux, *Jean de France*, 2: 290; Jones, *Ducal Brittany*, 128; Avout, *Armagnacs et Bourguignons*, 16-18; and Moranvillé, *Jean le Mercier*, 148.

⁶⁴ Avout, *Armagnacs et Bourguignons*, 13-17; Moranvillé, *Jean le Mercier*, 151-52; and Lehoux, *Jean de France*, 2: 292.

⁶⁵ Lefranc, *Olivier de Clisson*, 330-31; Lehoux, *Jean de France*, 2: 292-93; Moranvillé, *Jean le Mercier*, 159; Jarry, *La vie politique*, 96-99; and Vaughan, *Philip the Bold*, 43.

⁶⁶ See Rey, *Les finances royales sous Charles VI*, 577-78. Avout, a pro-Burgundian author, has written of Philip the Bold's laudable sense of moderation but has not dealt with the political considerations that must have compelled him to exercise this moderation; *Armagnacs et Bourguignons*, 57.

⁶⁷ *Ord.*, 7: 530-38; Nordberg, *Les ducs et la royauté*, 64; Lehoux, *Jean de France*, 2: 298.

⁶⁸ Morice, *Mémoires pour servir de preuves*, 2: cols. 577-78, 581-88.

⁶⁹ For the composition of the army of 1392, see the accounts of the royal war treasurer, BN, MS fr. 7858, ff. 309v-20v. Of the prominent military commanders whom I have classified as Bretons, only four served after 1393: Alain de Beaumont, who appeared briefly on the payroll of Louis of Orléans, and three members

although Clisson's heir apparent as the leader of the anti-Burgundian party based in northwestern France, was barely twenty years old and was not yet in a position to rally the followers of the disgraced constable against his powerful and experienced uncles.

THE KING'S INSANITY AND THE FALL OF THE MARMOUSETS set the stage for a profound crisis in the fortunes of the French monarchy, as factional strife in the early fifteenth century led to financial collapse and military defeat. The decade after 1392, however, was relatively calm, for the English were diverted by their own internal problems, and the rivalries of French princes did not immediately produce violence. At the same time, Louis of Orléans and Philip of Burgundy became antagonists on one issue after another, and the competing princes made increasing demands on the diminishing resources of the royal treasury.⁷⁰ The prominent commanders suffered some notable attrition, particularly as a result of the crusade that ended disastrously at Nicopolis in 1396.⁷¹ The surviving members of the group, however, particularly those from the north and west, became increasingly associated with the emerging Orleanist party. Among those identified as Orleanists by a scholar who has studied the duke's accounts are Guillaume de Melun, Jean de la Personne, Colart and Jeannet d'Estouteville, Yon and Jean de Garençières, and Guillaume Martel, several of whom were former Marmousets.⁷² Other sources, such as the large genealogical collections, indicate that an increasing number of royal officers were on princely payrolls after the mid-1390s, and the great majority were Orleanists.⁷³

The simmering factional strife first approached the character of a civil war in the fall of 1401. By this time Louis of Orléans had clearly emerged as the successor to his uncle Louis of Anjou and Olivier de Clisson in the leadership of the anti-Burgundian faction in France. His supporters were well entrenched in the kingdom's political structure. In the final years of his life, Philip of Burgundy made several dramatic, but largely unsuccessful, attempts to weaken his nephew's position.⁷⁴ It is possible to identify a number of

of the Mauny family, who also were Orleanists and whose lands were as much Norman as Breton. See note 29, above.

⁷⁰ Rey, *Les finances royales sous Charles VI*, 608–10; Avout, *Armagnacs et Bourguignons*, 41–55, 61–67; Lehoux, *Jean de France*, 2: 355–71; and Vaughan, *Philip the Bold*, 55–58.

⁷¹ For an interesting recent treatment of this expedition and what it meant for European international relations, see Palmer, *England, France, and Christendom*, 197–223. The prominent French commanders who died on this campaign included Philippe d'Artois, Guillaume des Bordes, Enguerrand de Coucy, Aimery de Rochechouart, Guillaume de la Trémoille, Guy de la Trémoille, and Jean de Vienne.

⁷² Nordberg, *Les ducs et la royauté*, 42–46.

⁷³ For documents linking commanders with Orléans in the 1390s, see note 61, above. For documents showing important commanders on the Orléans payroll after 1400, see BN, NAF 20027, nos. 213, 215; PO 246, doss. 5418, nos. 19–21; PO 493, doss. 11104, nos. 57–59; PO 494, doss. 11108, nos. 28–45; PO 577, doss. 13351, no. 2; PO 1896, doss. 43692, nos. 27, 32–34; PO 1668, doss. 38772, nos. 31, 32; PO 1561, doss. 35775, nos. 25, 29; PO 2209, doss. 49876, no. 13; PO 2057, doss. 46834, nos. 19–25. Since the king and the duke of Orléans shared households for extended periods, the abundance of documents placing people in Louis's service may be misleading. Yet these documents and the scores of others like them stand in striking contrast to the very few in the same collections that place important lords in Burgundian service.

⁷⁴ Nordberg, *Les ducs et la royauté*, 65–75. Nordberg's book is almost unknown in the United States. It suffers from the handicap of having been written before the author could have learned the results of

important military commanders as Orleanists in 1401 and 1402.⁷⁵ When Louis mustered his supporters in 1405, the list included many captains from the north and west, including the lords of Blaru, Cailleville, and Montenay, members of the Fayel, Garencières, and Trie families, and Robert de Béthune, viscount of Meaux.⁷⁶ Members of the royal council in 1406 and 1407 included fourteen names from our list of prominent commanders, of whom at least twelve were supporters of Louis of Orléans.⁷⁷

By this time, the dispute between Orléans and Burgundy had become extremely bitter. When Philip the Bold died in 1404, his son, John the Fearless, was at a disadvantage, not only because he lacked his father's prestige, but also because he seems to have been a less attractive personality. The queen and the aging duke of Berry apparently disliked him, and they moved gradually into the Orleanist camp.⁷⁸ John found himself increasingly deprived of revenues from the royal treasury, which had become an important part of the Burgundian budget.⁷⁹ In the spring of 1407, a reduction in the size of the royal council amounted to a veritable purge of Burgundian supporters and left Orléans in complete control of the French government.⁸⁰ It was in these circumstances that John engineered the assassination of the duke of Orléans, plunging France into a civil war that left the kingdom helpless in the face of a renewed English assault. The leadership of the Orleanist party passed to Bernard of Armagnac, a Gascon lord who had rendered homage to Louis in 1403 and whose daughter married the son of the slain duke.⁸¹ No longer centered in northwestern France, the Armagnacs, as the party was now called, lacked the strong regional base that had enabled Clisson and the Marmousets to triumph over Philip of Burgundy twenty years before. Despite the initial indignation at his action, John of Burgundy grew steadily stronger after the death of his rival.

The destruction of the French army by the English at Agincourt in 1415, and the subsequent English conquests in Normandy and the west, were major disasters, both for the financial structure established under Charles V and for the nobility of northwestern France. Many members of the regional aristocracy died at Agincourt and the Armagnac party was fatally weak-

research being undertaken at the same time by scholars like Rey, Vaughan, Lehoux, Palmer, and others. It is nevertheless a valuable corrective to Avout's pro-Burgundian study, and many of Nordberg's findings are corroborated by documents in the genealogical collections that he had no reason to consult.

⁷⁵ Guillaume le Boutellier and Guillaume de Fayel were important commanders in Louis's service whom he summoned to arms in 1401; Jarry, *La vie politique*, 262. Nordberg has identified a number of others who were in the pro-Orléans majority of the royal council in 1402; *Les ducs et la royauté*, 23.

⁷⁶ AN, KK 267, f. 109.

⁷⁷ Nordberg, *Les ducs et la royauté*, 222–23. Of the fourteen councillors who are also on our list of prominent commanders, only one, Guichart Dauphin, was incontestably pro-Burgundian. Nordberg assigns to neither party a member of the Estouteville family who assuredly was Orleanist, and he calls Pierre de Villaines a probable Burgundian supporter, although this old Marmouset (who died during 1407) is not likely to have supported John the Fearless.

⁷⁸ Avout, *Armagnacs et Bourguignons*, 74–82; Rey, *Les finances royales sous Charles VI*, 598–603, 611; L. Douet d'Arcq, *Choix de pièces inédites relatives au règne de Charles VI*, 1 (Paris, 1863): 283–85.

⁷⁹ Richard Vaughan, *John the Fearless: The Growth of Burgundian Power* (London, 1966), 41–42; Nordberg, *Les ducs et la royauté*, 34–35.

⁸⁰ Nordberg, *Les ducs et la royauté*, 219–23.

⁸¹ Vaughan, *John the Fearless*, 43–48, 82–98.

ened.⁸² The crown of France no longer had control over the very regions that had provided the military leadership during the French recovery of the 1370s. Although conditions were very different from the second quarter of the fourteenth century, this newest period of French abasement resembled the earlier one in that Burgundy was ascendant and the French crown received little support from the nobility of northwestern France. Contamine has pointed to the absence of much of the French nobility from positions of leadership in the royal service and the resulting dependence on foreign adventurers.⁸³ The political history of taxation also suggests that the nobility played a reduced role in this period. The Estates General in the 1420s and early 1430s made several attempts to restore the *aides*, but in nearly every case provincial estates then replaced these indirect taxes with *tailles*. Apparently, the nobles were so weak at the provincial level that the regional assemblies were dominated by bourgeoisie, who disliked the *aides*.⁸⁴

The geographical profile and political behavior of the military class in the generation that followed Agincourt still awaits study. It is evident, however, that the next French recovery was led militarily by another constable of Breton origin who had an important following among the northwestern nobility.⁸⁵ The restoration of the *aides* on a permanent basis after 1436 suggests a revival of noble influence relative to that of the bourgeoisie, particularly since it was specified that nobles pursuing a military career did not have to pay the *aides* on the produce of their lands.⁸⁶ Finally, the establishment of the *compagnies d'ordonnance* in 1445 can be seen as marking a close new alliance between the crown and the nobility. The army of Charles VII and his successors eliminated most of the brigands, bastards, and low-born people who had served during the thirty years after the battle of Agincourt. Increasingly dominated by middling nobles of legitimate birth,⁸⁷ the royal army provided regular and respectable employment for these people and mobilized the military class as an essential element of royal power.

THE EMERGING FRENCH STATE RESTED upon twin foundations: a solid financial structure and the political support of the nobility, especially those from the

⁸² Perroy, *Hundred Years War*, 238–44. La Roque gives an incomplete list of those who died at Agincourt. From our list of prominent commanders, the dead included the lords of Baqueville, Boissay, and Raineval; Jean de Garencières, Guichart Dauphin, Jean le Seneschal d'Eu, Charles d'Ivry, Jean de Montenay, and Guillaume de Melun. The count of Harcourt was among the prisoners. Three members each of the Craon and Rambures families were killed; *Histoire de Harcourt*, 3: 764–66.

⁸³ Contamine, *Guerre, état, et société*, 250–70.

⁸⁴ On the Estates of this period, see Major, *Representative Institutions*, 25–32. Regarding the preferred form of tax, see the comments in Henneman, *Royal Taxation*, 1356–70, 303–09.

⁸⁵ Arthur of Richemont, who became constable in 1425, may not have been inspired by the tradition of Du Guesclin as his biographer claimed; Cosneau, *Connétable de Richemont*, 356. But, as the son of John IV of Brittany and related to the Burgundian dukes as well as to the Laval, Craons, Clissons, and their connections, he was admirably situated to heal the wounds of a generation of factional bitterness.

⁸⁶ *Ord.*, 13: 211–15. The same phraseology had been used in 1393 when the uncles may have wished to cultivate the nobles after the fall of the Marmousets; *Ord.*, 7: 524–27. Reintroduction of this principle was surely a gesture of good will or appeasement to facilitate the resumption of the *aides*.

⁸⁷ Contamine, *Guerre, état, et société*, 400–04, 550–51.

north and west. There is no longer any basis for claiming that shrewd kings bypassed the great nobles, relied on low-born advisers, and with their help imposed a “modern” system of taxation. Monarchs, however able, were weak when they lacked adequate revenue and did not have strong support from the northern and western nobles. They were strong when they had such revenues and political support. Regular taxation came about mainly because of military circumstances—at first the capture of John II and later the pervasive problem of brigandage. The proceeds of regular taxes went mainly to the nobles in the form of salaries and pensions for military or administrative duties. When princely factional strife or military disaster weakened and divided the nobility (as in 1328–58 and 1401–35), it also weakened and divided the kingdom. If the successful monarch needed regular taxes in order to finance an army, he also needed the loyalty and collaboration of the military class, on which that army, and indeed the entire structure of government, depended.

Reviews of Books

GENERAL

PETER LASLETT. *Family Life and Illicit Love in Earlier Generations: Essays in Historical Sociology*. New York: Cambridge University Press. 1977. Pp. vii, 270. Cloth \$29.50, paper \$8.95.

Many of the essays in this book already have been published separately in scholarly journals. Their collection in this volume, however, is useful in unifying Peter Laslett's work on related topics and permitting an examination and evaluation of the author's methods, assumptions, and conclusions.

The book's title is misleading since it contains little explicit information about family life or illicit love, that is, about the meaning and context of human relationships. Except for the long first essay which mounts a "theory" about "the characteristics of the Western family," the book's focus is on specific demographic or structural topics. Most of the evidence comes from English sources but comparative data are introduced from French, German, and other local studies.

"Clayworth and Cogenhoe" is based on an analysis of rectors' books from mid-seventeenth-century England. The article finds that most villagers lived in nuclear family groups which also contained servants and that there was frequent turnover of population largely, though not exclusively, due to the migration of servants. A comparison with eighteenth-century French villages reveals similar household organization and slightly lower rates of turnover.

"Long-Term Trends in Bastardy" is a study of illegitimate births in England from 1550–1950. Laslett finds "well-marked regional variations"; no necessary correlation between late age at marriage and high rates of illegitimacy; and a pattern of rise and fall in bastardy rates before about 1680, when a steady increase, with little variation, began. The article also suggests that a subgroup of "bastard-bearers" may have been an important source of illegitimate births in some areas. These socially marginal people produced "not all the bastards," but a significant number of them. (This suggestion

has been documented in an interesting study by Keith Wrightson and David Levine, forthcoming in another collection of essays edited by Laslett.)

"Parental Deprivation" dissents from contemporary discussions of divorce and separation that suggest that single-parent families are a "modern" development. Using figures from Clayworth and other communities in the seventeenth and eighteenth centuries Laslett argues that parental deprivation is not new. Anywhere from one-third to one-fifth of children in preindustrial European society were "orphans," unmarried children having lost one parent, usually a father. Laslett does not address the question of whether there are differences—despite numerical similarities—in the experience of orphans in the past and of children of divorced parents in the present. He implies instead that similar structures give rise to similar emotional relationships.

"The History of Aging and the Aged" also takes issue with recent discussions, this time of the plight of the old in twentieth-century society. Laslett finds little evidence in lists of household membership that the aged "were always cherished by their families and kin-folk in the pre-industrial era." From this evidence, Laslett concludes that the relationships between elderly parents and their children have not changed much since the seventeenth century. What has changed, Laslett argues, is life expectancy and, therefore, the proportion of old people in the population. He concludes that social policy for dealing with the aged today must be "innovative." It cannot look for models to the past.

"Age at Sexual Maturity" traces the fall in the age of menarche since the Middle Ages. Laslett notes a good deal of variation over time, from class to class and place to place. He cites studies, particularly the important one by Rose Frisch, which indicate that nutrition is a key determinant of the onset of menstruation. Laslett finds that improvements in nutrition in European society since the 1890s have diminished the variations in the age of menarche among classes. He notes, too, a difference between the age of menarche and the age at

marriage in the past. Sexual maturity was a precondition for marriage, but attainment of it did not necessarily or immediately lead to marriage. Thus, the import of the entire article for the history of the family, while implied, never is made entirely clear.

The final essay deals with recent research on slave families in the United States, particularly with *Time on the Cross* by Robert Fogel and Stanley Engerman. Laslett attempts to compare slave "families" with those he has studied in pre-industrial Europe, but the vastly different social and historical contexts make for a superficial exercise at best. He finds that "parental deprivation" for slave children in the formative years of their lives (from infancy to five years of age) differed little from that of children in English peasant households. This leads to the suggestion that patterns of socialization may also have been similar. Yet without an exploration of the social, political, and cultural context of socialization the inference seems unjustified.

The first essay provides the theoretical overview for the other articles in the book. In it Laslett offers four characteristics of the Western family before industrialization. First, it was a nuclear unit. Second, mothers of these families married at a relatively late age. Third, there tended to be only a few years difference in the ages of spouses. Laslett suggests the marriages tended to be more "companionate" as a result. Fourth, servants were often present in the households of nuclear families. These four characteristics, says Laslett, had an important impact on the socialization of children, and therefore on the personalities of the inhabitants of Western Europe. There are no details offered, however, about the nature of that socialization or the personalities it created. Laslett leaves that discussion to experts on child psychology. But his theory assumes that characteristics of household organization are a key to family and kin relationships and that few other factors matter. Laslett's critics have pointed repeatedly to the limits of this assumption. It does not consider the functions of household arrangements for families nor does it deal with rules for inheritance and reproduction. Not simply the age of marriage partners, but the social rules about property transmission and reproduction, will determine whether the relationship between husband and wife is "companionate" and what the affective ties are between parents and children.

The "theory" has other limitations as well. It is based almost entirely on English evidence and so is essentially a generalization from England to all Western Europe. In addition, no attempt is made to analyze systematically differences of family experience among different classes or occupational groups. The reliability and representativeness of

the sources are also problematic. Laslett constantly reminds his readers that the data are "imprecise," "incomplete," "partial," "truncated," and cannot be seen as "decisive." Yet his argument assumes the greater scientific precision of numerical evidence to attitudinal or literary evidence. Indeed, he attempts to build a theory about the socialization of children and the uniqueness of the Western personality by inference from incomplete statistics about such things as household size and age at marriage. He insists in his introduction that his method of historical sociology is far superior to any analysis of literary evidence from the past. Yet ironically the criticism he applies to literary evidence is applicable equally to Laslett's numerical data: "So untidy is it, so variable and contradictory in its dogmas and doctrines, so capricious in what it preserves and what it must leave out. . . . To infer from such evidence what the whole content of the attitude to children was amongst the elite minority would itself be an uncertain task. . . . And to attempt to go further and reconstruct on this basis the childhood experience and the childrearing practice of a whole society . . . would be formidable indeed" (pp. 18-19).

Laslett's descriptions of his data are interesting and suggestive, but they do not provide a solid base for a theory of the Western family. The theory is without sufficient grounding in the historical and cultural contexts of love (illicit or licit) and family life.

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ARTHUR E. IMHOF. *Einführung in die Historische Demographie*. (Beck'sche Elementarbücher.) Munich: Verlag C. H. Beck. 1977. Pp. 149. DM 16.80.

Probably the earliest historical demographic study of a parish based on the technique of family reconstitution was published by a German scholar in 1907. Later, in the 1920s and 1930s, dozens of similar studies emerged in Germany. It is thus particularly ironic that historical demography lay dormant during the postwar decades when family reconstitution was being reinvented in France and generating such excitement among social historians and demographers alike throughout much of Europe and North America. But historical demography in Germany during the Third Reich became inextricably entwined with Nazi racial ideology and was virtually taboo as a scholarly pursuit for almost a generation of postwar German academics. In *Einführung*, Arthur E. Imhof attempts to redress the situation. He intends not only to in-

form his German readers that historical demography has been booming elsewhere, but also to whet their appetites to take up the cause at home and exploit the unusually rich resources Germany has to offer. He may well be successful.

This short book consists of four chapters. After a very brief introductory chapter, Imhof traces the development of modern historical demography, mentioning the work of the most prominent post-war scholars in France and England in a generally engaging manner. But it is his chronicling and description of the closely related approaches which emerged in Germany even earlier that is the most fascinating and original contribution of the volume. In this chapter he also calls attention to the wealth of source material available in Germany.

The third and longest chapter portrays modern historical demography as a "problem-oriented discipline" and is a sampler of substantive findings from a remarkably wide variety of studies, some already classics, others much less well known, including some of Imhof's own work. Imhof attempts to go beyond merely cataloguing the findings by weaving them together in a clever, if not always convincing, manner. Although the "story line" does not always stand up to rigorous scrutiny it should still succeed in capturing the interest of uninitiated readers and demonstrate how extensive the potential of historical demography is for illuminating social behavior in the past. The fourth and final chapter discusses methods of historical demography and is limited to a contrast of aggregative analysis and family reconstitution. He provides some concrete examples intended to give the reader a feeling for the type of data processing each approach involves.

Throughout the book, Imhof makes generous and often effective use of graphs, charts, and tables. Occasionally, however, the charts are so detailed (e.g., figures 12a and 12b) that most readers will probably have trouble digesting them in a meaningful way. He seems to be most at home when discussing mortality and areas related to medical history. He does not seem to understand fully the concept of natural fertility (n. 85) nor to assign it the prominence I believe it deserves. His treatment of the demographic transition is also unsatisfactory. His assertion that both age at marriage and illegitimacy rose during the demographic transition is misleading; during the fertility decline phase, just the opposite is closer to the truth.

In general, the book's strengths outweigh its weaknesses. The usefulness of *Einführung* for scholars outside the German-speaking countries will be limited since it is written in German. Also, it is mainly a review of others' research and not a major original contribution in itself. But Imhof's en-

thusiastic style should help instill a greater interest in historical demography among German students and scholars not yet familiar with the field and introduce them to the rapidly expanding wider international body of research in the discipline. And this indeed is the author's intention.

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JEFFREY BURTON RUSSELL. *The Devil: Perceptions of Evil from Antiquity to Primitive Christianity*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 276. \$15.00.

Jeffrey Russell insists repeatedly that his latest book is a work of history and not of theology, but many readers will wonder if it is not really a curious blend of both. The author sets himself several major tasks. First he attempts to evoke a sense of the reality of evil in the world, finding examples as widely scattered as Ashurnasirpal, Dostoevsky, and the Nazi death camps. Russell defines evil flatly as the "abuse of a sentient being, a being that can feel pain. It is the pain that matters" (p. 17). Ignoring many of the possible objections here, Russell next employs a Jungian argument to argue that evil, if merely repressed, forms a shadow in the psyche which can later erupt dangerously. The healthy response confronts evil, "not by repression, which only increases the shadow of the unconscious, but by conscious suppression of the evil elements that we have recognized in ourselves" (p. 32). I take it that Russell's purpose in this book, therefore, is to persuade modern man to take the devil seriously as a personification of evil and to reconsider the problem of theodicy in which a devil may be a helpful character. Accordingly, Russell's history attempts to show that many ancient cultures moved from a kind of naive monism, in which the gods are good and evil by turns, to a more and more pronounced dualism, in which God has become so perfectly good that he evokes (or requires) a devil as an equal counterpart.

It is true, of course, that not all religions or cultures developed along these lines, and in his third chapter Russell provides miniature sketches of evil in non-Western religions, from Buddhism and Hinduism to the Hittites, Babylonians, Egyptians, and Canaanites, and on to the Aztecs, the Winnebago, and the Kogi of the Andes. The chapter tries to show that even monist systems display dualist symptoms. In the end, however, Russell concludes that monism is complacent because it imagines a hidden harmony of good and evil (p. 228). On the other hand, a sharp dualism in religion "abets repression, rather than healthy accept-

ance and conscious suppression of violence" (pp. 100-01). Still, Russell is grateful to Iranian dualism for insisting "upon the existence of an absolute and radical evil. Not only does this in part respond to our perceptions of the world, but it for the first time limns a figure clearly recognizable as diabolical" (p. 101). When he turns to the ancient West, Russell perceives a parallel emergence of Neoplatonic dualism from the ambiguous mists of the classical pantheon. Again, one can object that this oversimplifies a complex topic, and Russell even notes that he ignores Aristotle partly because he actually turned back from dualism to a form of monism (p. 149).

Russell's treatment of the Judeo-Christian perception of evil covers the well-known rise of Satan as a figure of power and independence, from the monism of the Pentateuch to the emerging dualism of Job, the intertestamental apocalyptic literature, and the New Testament. Arguing effectively that Christianity presents a creative tension between monist and dualist temptations, Russell insists that the devil is crucial to New Testament Christianity. Even if the generous reader concedes this point without contest, it still seems that Russell is far from having proved that the devil was (or is) necessary for Christianity as such. Many readers will worry that in such arguments Russell taxes history beyond its ability to pay. Before one joins Russell in leaping from the putative universal experience of evil to a natural diabolology (p. 260), and before one revives the devil in the interests of mental health, one should expect a more rigorous argument, a more cautious psychology, and perhaps a greater willingness to concede that a satisfactory theodicy may indeed be beyond human comprehension.

II. C. FRIK MIDELFORT
University of Virginia

ALAN THOMAS. *Time in a Frame: Photography and the Nineteenth-Century Mind*. New York: Schocken Books, 1977. Pp. 171. \$17.95.

It is only within the last few years that historians have begun to explore the potentialities of photography as a source of historical insight and documentation. Those potentialities are significant for the many themes that intersect in photography: technological and business development; social and cultural values; esthetic expression; and scenes of work and habitat rarely recorded by other art forms.

Alan Thomas' short book deals mainly with Victorian England and explores the scenic documentary, the family album, celebrity and theatrical photography, and the emergence of the social

documentary. No subject is covered in depth, and one does not gain much insight into the "nineteenth-century mind" or the history of photography. The book's value inheres in its demonstration of the use of photographs as historical documents—the photograph (or group of photographs) becomes the point of departure for speculation about the ideological or social predispositions which favored certain kinds of subject matter and esthetic conventions. The author points out, in this connection, that decisions as to subject and form were always greatly influenced by nontechnical considerations—the self-image of a nation, class, or occupational group shaped the content of photographs as much as problems of bulky equipment or the troublesome emulsion preparation required by the nineteenth-century collodion wet-plate method. Particularly significant was the Victorian passion for social respectability; whatever artistic pretensions the commercial photographer may have possessed, they were overwhelmed by the determination of working-class as well as upper-class clients to strike a posture of respectability and propriety in the gallery.

Along with social respectability, Thomas touches on such themes as romanticism, nationalism, sentimentality, and escapism as cultural foundations for the content of Victorian photography. Although the author's effort to probe the intersection of photography and culture is often speculative and tenuous, the speculations are reasonable and imaginative. And one can readily subscribe to his proposition that photography which has little value as art (such as much Victorian portrait photography) does possess great value for purposes of historical detail and documentation as well as cultural analysis.

Thomas does not deal with two issues which are basic to any thorough examination of photography and society. An art form is profoundly influenced by the external society—its values and needs. But every art form has an internal history as well; it is shaped by the artist's or practitioner's vision of himself in relation to his predecessors and contemporary artistic ideals. Thus it was not only technological-business imperatives, or broad cultural ideals, which shaped nineteenth-century photography; it was also influenced by extensive agonizing over whether photography was an art form, its relationship to painting, and the photographer's quest for status. The second issue concerns the relationship between art and reality or consciousness. While Thomas focuses, legitimately, on the influence of Victorian culture on photography, art and photography also shape culture. Our sense of reality is influenced, if not created, by art (and literature). How many, for example, who came of age after the 1930s view the era through

the rectangular frame of a Walker Evans photograph, or the pages of Steinbeck's *Grapes of Wrath*?

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MARTIN VAN CREVELD. *Supplying War: Logistics from Wallenstein to Patton*. New York: Cambridge University Press. 1977. Pp. viii, 284. \$14.95.

Martin Van Creveld of the Hebrew University, Jerusalem, reminds us that although logistics embrace about nine-tenths of the business of war, for every hundred books on strategy or tactics, perhaps one may be found dealing with the topic of supplying war.

This slim volume, unique of its kind, not only iterates the value of the study of logistics to the understanding of any war, any campaign, or any battle, but presents significant historical reinterpretations and revisions on practically every page. Military historians, like their counterparts in other research fields, tend to take in each other's laundry, but Van Creveld has consistently gone to his sources.

What other historian, for example, would teach or write that in the 1866 Austro-Prussian War the Prussians "behaved as if the railways did not exist," and that in 1870 France's railroads were superior to those of the German states? (The superiority of French railroads should also remind readers that French armor in 1940 was, in most cases, the best in the world.)

Van Creveld follows his selected campaigns in such detail and in so masterly a fashion that it proves difficult to argue with even his most upsetting conclusions. He devastates the famous Schlieffen Plan for slighting logistics in favor of strategy, and notes that "German utilization of national resources, especially manpower, was nowhere as comprehensive as that of France." Although Germany's population exceeded France's by some two-thirds, the General Staff was hard pressed to achieve even parity of military forces. As for the imagined German meticulousness in planning, it becomes clear that von Schlieffen counted on nonexistent corps, on the stripping of reserves, and on a totally fantastic "shoulder-to-shoulder advance along the 350 miles from Verdun to Dunkirk" (pp. 134-35). Clearly, more is needed for an understanding of the planning for war in the twentieth century than anecdotal tales of the superiority of German field kitchens, or unsupported general assertions.

The Germans proved no better in planning for World War II. "The Army that shook the West was a scavenger" (p. 146), picking up vast troves of French, Belgian, British, Czech, and Dutch equipment to supplement a totally inadequate domestic

motor industry. And, as is well known amongst military historians, much Wehrmacht transport was horse-drawn. What a contrast all this poses to the completely mechanized British Expeditionary Force, or to the French army of 1940, for that matter.

For the invasion of Russia, the Wehrmacht transport pool numbered no less than two thousand *types* of vehicles, ranging from primitive peasant *panje* carts, to futuristic rear-engined Czechoslovakian Tatra staff cars. A mere month's reserve of munitions was allotted for the greatest military operation of all time, while logistic troops were actually to precede operations, all of which moves the author to exclaim that "the German General Staff seemed to have abandoned logical planning for *Operation Barbarossa*" (p. 151). The General Staff did indeed lay in winter equipment (another myth demolished), but was unable to forward it to the troops.

Poor logistics also destroyed General Irwin Rommel. The much-maligned Italians were able to ship practically everything the Afrika Korps needed, but, again, the troops failed to receive much of it, and not surprisingly, as the distance between Tripoli and Alexandria is about twice that between Brest-Litovsk and Moscow, with North Africa suffering from even worse transport facilities than those found in western Russia.

Van Creveld does not spare the Allies. Planning for the invasion of Northern Europe was precisely the opposite of that of the Germans; Allied planning was excruciatingly meticulous, and Allied resources several times over more abundant than those of their enemy. Yet the Normandy breakout to the Seine was hobbled by "logistical pusillanimity," a fearful unwillingness to utilize overwhelming superiority.

In sum, this work has demolished some enduring historical myths. But most important, it should demonstrate that mere detailed planning is not enough. As Moltke the elder put it: planning should not extend beyond the first encounter with the enemy. If this classic work has its faults, they lie in certain things left undone. For example, logistical planners might have come out of this book somewhat better had the author studied the great and successful logistical operation which supplied American forces thousands of miles from home in the Pacific during World War II. Further, the author often appears unwilling to look to "the other side of the hill" to see what the other side was doing with its logistics. Were they making even worse blunders? Not always. We know that the British Expeditionary Force left no *panje* carts behind on the sands of Dunkirk.

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RICHARD SMOKE. *War: Controlling Escalation*. Cambridge, Mass.: Harvard University Press. 1978. Pp. xvi, 419. \$18.50.

In this insightful and creative book Richard Smoke combines theoretical and empirical approaches to enlarge our understanding and ability to control the escalation of wars. The book is divided into three sections concerned with problem formulation, comparison of historical cases, and theory development. In the short first section, Smoke develops a "model" or "working concept of escalation" which he defines as "an action that crosses saliency which defines the current limits of a war, and that occurs in a context where the actor cannot know the full consequences of his action, including particularly how his action and the opponent's potential reaction[s] may interact to generate a situation likely to induce new actions that will cross still more saliencies" (p. 35). Among the three general approaches to war (scientific, moral, and operational), Smoke sees his primary goal as the last, namely, "results that might be *operationally* useful to civilian and military officials during wartime. The research therefore should be primarily directed, not at the creation of an abstract theory about escalation control, but at the uncovering of concrete factors and issues that might prove relevant and applicable in practice." He nonetheless also "want[s] to make a contribution toward filling a very large gap . . . in empirical study of escalation" by basing the work on "known or discoverable facts about the way escalation has worked in past wars, and how it has been controlled or failed to be controlled during those conflicts" (p. 36). Among possible approaches he chooses a variation of the case study method designated as "focused comparison," i.e., specification of a limited number of instances among which "the pattern of events" are compared to reveal similarities and differences. As a basis for this comparison he derives questions from his working concept of escalation that concentrate on decisionmakers' perception of salient limits, expectations about future developments, goals, responses to escalation by opponents, and expected and actual consequences of escalation.

The individual cases are examined in the long middle section. Smoke elects pre-Cold War wars because—unlike Cold War conflicts—they can provide information on both sides and display features of many contemporary wars. He rejects those in which escalation was either virtually unavoidable or highly unlikely and chooses as most instructive those "in which the control of escalation has strikingly failed where it might have succeeded, and cases in which it has strikingly succeeded where it would be understandable for it to

have failed" (p. 43). On this basis he decides upon the Spanish Civil War, the Austro-Prussian War, the Franco-Prussian War, the Crimean War, and the Seven Years' War (1756–63). In each instance he provides a historical overview of the war's background, the "escalation sequence," the "pattern of escalation," "the sources of escalation control," and an "analytic summary."

In the final section Smoke draws a number of conclusions. He concedes that the most obvious is the great variation which both highlights significant specific aspects of escalation and confirms the assumption that it is complex and difficult to control. Nonetheless, a central conclusion is the "high importance in controlling escalation of the framework of conflicts" (p. 245), i.e., the concrete historical situation which is sometimes overlooked by those who concentrate on the bargaining aspects. The specifics of the historical setting create what Smoke calls "the irregularities in the pattern of a conflict's potential escalation. . . . [Thus] there are different ladders for every conflict, and they bear only a loose resemblance to one another" (p. 251). He argues that "conceptual failures," i.e., mistakes in comprehending the historical context, played "a dominating role in those [cases] where escalation got out of control" (p. 253). A frequent contributing factor was the failure to recognize that not one but a series of events might follow from a decision. Perhaps the most critical element is the tendency of policy-makers "to underestimate how much their opponents' expectations [about future events] differ from their own" (p. 277).

The work has a number of interesting implications. Smoke's desire to aid policy-makers is creditable even if not credible to many historians. He is appealingly humble in recognizing the difficulties involved in understanding and controlling escalation. He furthermore acknowledges that control can be converted into manipulation, i.e., exploitation rather than avoidance of escalation. Above all, the notion of deriving and applying lessons from the past is a slippery one about which most historians have serious doubts. Nonetheless, it is difficult not to share Smoke's concern about escalation in a nuclear age and his willingness to think about it in historical terms.

The book can serve as a model for interdisciplinary studies. It is well organized, thoroughly researched, generally eschews jargon, and moves easily with an almost conversational style; where specialized terms are used, they are clearly defined and appropriate to the context. Most important, the book demonstrates that political scientists and historians are engaged with the same problems and can aid one another. The comparative methodology of political scientists can provide historians with insights into common and

uncommon features of specific events. Smoke's model for the escalation of international wars is applicable, at least in a general sense, both to prewar diplomacy and to phenomena—such as civil wars and revolutions—in which escalation plays a significant part. Conversely, history can be useful to political scientists not only as a source of data but also as a healthy reminder of the complexity of actual events. It is perhaps particularly gratifying to diplomatic historians in a moment when their specialty is out of fashion to be reassured by a political scientist that diplomatic history may be helpful in avoiding that most catastrophic of eventualities, atomic war.

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HUGH SETON-WATSON. *Nations and States: An Enquiry into the Origins of Nations and the Politics of Nationalism*. Boulder, Colo.: Westview Press. 1977. Pp. xv, 563. \$25.00.

Hugh Seton-Watson of the University of London is especially qualified to do this big book. Uniquely educated from his early youth by family associations and by study and travel in many lands, he is amazingly informed. Commanding several languages, he has read widely not only in Russian history, his specialty, but in the histories of many peoples.

Going far beyond his earlier studies of Eastern Europe, he here sets out to examine "the processes" throughout the world "by which nations have been formed. . . ." This is a formidable task. The resulting book, densely packed with facts, observations, and interpretations, both dazzles and puzzles. It also demonstrates Seton-Watson's belief that "the interplay of personalities, institutions, ideas, impersonal forces and mere chance is exceedingly complex and infinitely variable" (p. xiv).

In his early pages he clearly distinguishes between states and nations, terms which he rightly declares are often confused. Then in twelve uneven chapters he covers nations, states, and national movements everywhere. Few escape his scrutiny or at least mention. Beginning with the European nations and those established by Europeans overseas, for example, in the Americas, he goes on to the many varieties of Asian and African nations. His accounts of southeast Europe and Russia are particularly informative, while those devoted to France, the United States, and Africa are less so. In his last chapters he comments on race and nation in the cases of the blacks and whites in the Americas and South Africa, and the Indians in the Americas—especially the white "crimes"; on the

diaspora nations—not only the Jewish; on class and nation—the varied origins of the nationalist elites; on nationalism and the ideologies of socialism, fascism, and Communism; and finally on the divisive nature of the human community. One could reasonably argue that in these last chapters he might profitably have discussed other questions than those he chose.

In the formation of nations he finds "in each case a different combination of constantly recurring forces: state power, religion, language, social discontents and economic pressures" (p. 10). Perhaps his major contribution is his exhaustive demonstration of the complexity of the interactions of these and other forces. But his contributions are varied and numerous, among them the meticulous analysis of the differing class origins and natures of nationalist elites, and the scattered penetrating insights he offers. For instance, analyzing ideological movements he concludes, "There was a nationalization of communism, a Marxisation of nationalism, and a predilection by adepts of both to the style of Mussolini" (p. 461).

These insights, on the other hand, appear in "bald" narratives in which facts and opinions tumble rapidly, at times pell-mell, after each other while too often their relevance is not clear. Given the comprehensive nature of this book, criticism is inevitable. Two points of minor importance: there are infrequent errors of fact—Hitler did not conquer "the whole of Europe" (p. 99); and in the bibliography many important books are not listed, while others of general nature or peripheral interest are. Questions of more serious import must be raised. Should not Seton-Watson, sacrificing some of his spectacular display of details, have placed more emphasis on the efforts of leaders of states to build nations internally through economic policy and practice, education, and propaganda as well as on the demands of people, ordinary as well as extraordinary, for strong states and national unity? Certainly, too, his conceptions (not always consistent with but sometimes influenced by Elie Kedourie) of nationalism may be challenged. Nationalism, he writes, is "a movement for national independence or national unity or a policy of creating national consciousness within a politically unconscious population" (p. 449). With this limited definition Seton-Watson can surprisingly assert (p. 469) that nationalism "played no part" in the motivations for United States intervention in Korea and Vietnam!

It is perhaps not surprising that serious interpretations of nation-forming and of nationalism should differ. As Seton-Watson vividly shows, the phenomena are complex and variable, and scholars are of many differing persuasions. When this encyclopedic book is mined and challenged, as it

will be, the author's own predilection should be noted. Skeptical of unity beyond the national within the human community, he believes "rejection of national consciousness and national heritage a sign of political sickness . . . an act against civilization" (p. 481).

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RICHARD J. EVANS. *The Feminists: Women's Emancipation Movements in Europe, America and Australasia, 1840-1920*. New York: Barnes and Noble. 1977. Pp. 266. \$17.50.

This effort by Richard J. Evans, a British scholar, attempts to analyze the history of feminism throughout the Western world. Of necessity the author has had to rely mainly on published sources. As always in such undertakings, the results are uneven. Evans is most useful when talking about German feminism, the subject of a previous book. He is least original and reliable when attempting to deal with American feminism. Even so, parts of this book are rewarding. Evans is good on the complex relations between feminists and socialists. His brief comparison of the ideology of women's liberation with that of the older feminism is suggestive and possibly even right.

Where the book fails it is not so much from lack of knowledge as from dogmatism. Evans means to establish an ideal model that, with some exceptions, will encompass the rise, and then, after World War I, the decline of the entire women's rights movement. He maintains that feminism arose from the new economic dependence of middle-class women in the nineteenth century, and was focused initially on economic issues. Progress in this area led to a broader concern with moral issues, chiefly intemperance and prostitution. This in turn led to radicalism, the demand for the vote, followed by an upsurge of conservatism, fragmentation, demoralization, and collapse. He further insists that the only possible term for the first phase of the women's rights movement is "moderate," while the later, more politicized, movement can only be called "radical."

Anyone familiar with the history of feminism in either America or England will see at once that this model does not apply. American feminism had its roots in the temperance and antislavery movements. English feminism evolved from campaigns against legalized prostitution, a crusade that owed little to economic interests, much to moral outrage. The concern with economics, where it existed, came later. Nor does Evans' contention that feminism involves a progression from moderate to

radical hold water. American feminism was radical in its early phase, after which, for reasons that Evans is familiar with, it lapsed into moderation. Evans tries to shore up his framework by ruling out or redefining what does not fit easily into it. Thus the demand for the vote, if made early in a movement's history, is "premature" and proves nothing. The alleged conservatism of American social feminists in the 1920s is supported with the claim that they continued to be antipacifist after the war, whereas in fact they did not. If all else fails, Evans will simply invent what his theory requires, as when he says that economic needs account for the origin of feminism. Evans is led into error and distortion because feminism was stronger in England and the United States than anywhere else in the world, and so events in these countries must conform to his general thesis if it is to succeed. But the Anglo-American feminist experience does not fit his pattern. Faced with this dilemma Evans has employed a familiar strategy: if the facts do not fit the theory, so much the worse for the facts. Accordingly, his book fails to persuade.

Having said this, I must also say that *The Feminists* is worth reading anyway. Evans is intelligent and well informed, on European feminism especially. And when not enslaved by his thesis he makes numerous interesting, and often carefully qualified, associations between different groups and eras. His title notwithstanding, Evans knows that one cannot speak of *the* feminists as if they were all of a kind. This saves his book, almost despite itself, though by what margin only further research will tell.

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NORA LEVIN. *While Messiah Tarried: Jewish Socialist Movements, 1871-1917*. New York: Schocken Books. 1977. Pp. xi, 554. \$24.50.

Nora Levin's newest book on Jewish history recounts the fate of Jewish socialist movements in the United States, Russian Poland, and Palestine from the late nineteenth century to 1917. She attempts to bring together standard English-language accounts of these movements with illustrative materials from Yiddish memoirs and other sources. The result is three readable but distinct accounts which might appeal to a college classroom audience or to the interested layman (unless dismayed by the absurdly high price).

A common theme of the time lay in the conflict between the universal outlook of socialism and the particularism seemingly inherent in a Jewish, per-

force Yiddish-speaking, socialist movement. In Levin's attempts to explain this situation, she goes far afield in discussing the spread of Jewish/Yiddish culture among American Jewish workers. The same conflict arose in Russia, where the Jewish Bund clashed with the leadership of the Russian Social Democratic Workers' Party. The latter struggle, chronicled so often elsewhere, appears in this book (as in so many other books) as a battle of party leaders. It is often difficult to know just what the Bund meant beyond the infighting at the very top levels of the parties. What did it mean as a social movement?

The section on Palestine is the weakest, in large part because a real socialist movement did not emerge there until the 1920s. The author is forced to stick to a few ideologues and to the beginnings of the kibbutz movement. The topics concern the very interesting attempt by the early Zionists to form the "new Jewish human being" through a new relationship to physical labor.

The most disturbing intellectual failing lies in the periodization: the author uses 1917 as a terminal date without pondering its importance. Did the Russian Revolution create a caesura in the life of these Jewish socialist movements? It did for the Bund but certainly not for the others. In fact, one can probably not understand any of these groups, including the Bund, without looking at the 1920s and the 1930s. In this period the Bund in Poland for the first time (except for a brief flareup around 1905) became a true mass movement; the Palestinian labor movement was created and had to forge its identity; American Jewish socialism became even more closely identified with the labor movement and went through many years of social struggle. The year 1917 is an improper choice for this endeavor, although it is only fair to note that this fascination with the early stages of social movements is widespread among historians. In this case the story is too truncated to be completely satisfying intellectually.

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ANTHONY D. SMITH, editor. *Nationalist Movements*. New York: St. Martin's Press. 1977. Pp. vi, 185. \$15.95.

Terms such as nationalism and nationality have exasperated a legion of writers who have tried to define and analyze the ideology represented by these words. Because nationalism is so wide-ranging and complex a phenomenon and has tended to invite strong value judgments, there has been little success in dealing with it in its entirety.

Anthony D. Smith, who already has established

a reputation as a discerning analyst of what he aptly describes as "a central feature of the processes of modern social change," has brought together a small group of scholars to attempt a comparative and interdisciplinary examination of nationalism. This undertaking is characterized by individual contributions rather than a team effort or an attempt at any collective view. As such, the essays in this work vary both in their scope of inquiry and value. Whatever thematic unity there is derives from the laudable efforts of the various authors to examine nationalist movements through the combined perspective of a time (historical) and space (sociological) framework.

Smith's introductory essay is the most inclusive in this regard. In some respects it is a summary of what he has already written in an earlier work, *Theories of Nationalism*. Yet it seems to me that Smith has sharpened and clarified his ideas and avoided some of the difficulties in terminology and classification that appeared in the earlier book. His essay here is lucid and compelling, and deserves a wide readership.

The spread of the national idea throughout the non-Western world has led to extensive research by social scientists into this phenomenon. Their theories have often yielded encouraging results when applied to the first independence movements in Europe. Such is not the case, however, with W. J. Argyle's piece on "Size and Scale as Factors" in nationalist movements. His use of anthropological terms—"proposing the category, elaborating the category, making subsidiary corporations, mustering the people"—in a study of nineteenth-century East European nationalism based exclusively on English sources reveals only the inadequacy of the theoretical framework. A chapter by Peter Calvert on what becomes of nationalist movements after the attainment of independence is more rewarding, drawn as it is from the non-European experience and directed primarily toward non-Western movements.

All societies have had to face the problem of maintaining group loyalty and cohesion. Two of the essays deal with this issue from widely divergent perspectives. T. R. Warburton's consideration of language as a factor for unity or disunity in the cases of Switzerland and Canada points up the importance of a society's unique historical experience in nation-building. Using the triptych of "patriotism, legitimism and nationalism," K. R. Minogue surveys the European past from the city-state (patriotism) to absolutist monarchy (legitimism) and then leaves the triadic image incomplete with nothing more than a sketch (p. 70) of the role of nationalism in modern communities.

Most studies of nationalist movements have tended to place emphasis on the role and ideas of

intellectuals. The relationship of class and economics to nationalism has concerned primarily Marxist writers. In a sweeping historical essay covering the world over a period of two centuries V. G. Kiernan explores this formidable and fundamental issue. His concise and sustained account raises perhaps the most basic question regarding nationalism: despite the violence and upheaval associated with it, has it proved to be a constructive force in the modern world? Kiernan answers in the affirmative for Asia but in the negative for the European continent. Recent events in Africa and Asia, however, would tend to cast doubt on this opinion. Overall, nationalism might be seen as a means by which individuals have tried to meet the demands made on them by the other ideologies with which it has been coupled.

Nationalist Movements is a commendable effort to increase our understanding of the most pervasive force in the world today.

GERASIMOS AUGUSTINOS
University of South Carolina

GIORGIO BORSA. *La nascita del mondo moderno in Asia Orientale: La penetrazione europea e la crisi delle società tradizionali in India, Cina e Giappone*. (Collana Storica Rizzoli.) Milan: Rizzoli Editore. 1977. Pp. 603. L. 10,000.

Until the Industrial Revolution contacts between Europe and Asia had more impact on the former than on the latter. Giorgio Borsa demonstrates this in his first hundred pages by surveying Europeans' cultural and intellectual responses to Indian and Chinese civilization up to the eighteenth century. Europe's impact on Asia became powerful after 1757 and the birth of modern Asia began. Borsa studies this process until the defeat of the last considerable resistance, i.e., until the Satsuma and Boxer rebellions and the events in India which he is Eurocentric enough to refer to as the Mutiny.

According to Croce, all true history is contemporary, directed toward understanding the present. Borsa believes that because of Eurocentric bias historians who wrote before 1939 throw little light on the origins of the "new Asia" of today. He therefore looks at their work with a fresh eye and surveys for the benefit of Italian readers the best postwar work available in English and French. In order to make such an account readable and coherent, imagination as well as scholarship is needed, and he sometimes ventures beyond his evidence. How does he know that Warren Hastings' revenue-farmers systematically falsified the records in order to secure their local influence? He suggests that Cornwallis adopted the Permanent Settlement in 1793 partly because he hoped that it

would increase Indian purchasing power to the advantage of British manufacturers, and cites pamphlets of the 1820s in support. This seems like historical determinism. A decision is to be explained not by the statesman's subjective understanding of the facts that faced him, but by the objective significance of his role, as shown not by the consequences of his acts but by the opinions of a later generation. In this way, indeed, all history becomes contemporary.

Borsa is a true historian, however, and knows that the light which history can throw on the present must be a by-product of the attempt to understand the past. His work is much more traditional than his introductory remarks make one expect. It is, after all, not true that writers such as Firminger, Baden-Powell, Morse, or Murdoch in the age of imperialism lacked interest in the spontaneous stirrings of Asian society, and the best recent work on the period has still had to be Eurocentric to some extent, since the dynamic forces to which Asia responded were European. The alternative so far has been speculation about the applicability to Asia of stage theories which were designed for Europe and assertions about the stage Asia might have reached by now if there had been no Western imperialism, and Borsa rightly ignores such literature. Since some of his research was done in India, Japan, and Hong Kong, it is surprising that he ignores journals such as the *Indian Economic and Social History Review*, which give the best view of the contemporary work of Asian scholars.

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W. DAVID MCINTYRE. *The Commonwealth of Nations: Origins and Impact, 1869-1971*. (Europe and the World in the Age of Expansion, number 9.) Minneapolis: University of Minnesota Press. 1977. Pp. xvii, 596. \$25.00.

History is rewritten by each successive generation; and this has proved especially necessary for what has been called "The Changing Commonwealth." Earlier histories of the British Empire stressed its expansion of territory and its beneficent dissemination of liberal institutions. In the 1930s, A. B. Keith wrote as if, with the Statute of Westminster, the ultimate possible development of the British Commonwealth had been reached in the shape of a voluntary association of white dominions under the crown. But the world moved on and the British Commonwealth with it. Post-World War II anti-colonialism transformed it into the Commonwealth of Nations, a voluntary association of most of the former British dependencies, now all independent and many of them republics. These had

retained their membership in what J. D. P. Miller aptly called a "concert of convenience," which only a small number had failed to join, or had left, or from which a couple had been ejected. Some recent reinterpretations have tended to assume that all this actually signifies the end of the Commonwealth. For instance, Max Beloff's first of two planned volumes was inspired by Britain's rejection of its world-wide relationships in favor of integration into Europe in the Common Market and an assumption that this marked a complete about-face. W. David McIntyre, a Briton who now teaches in New Zealand, has wisely avoided the conclusion that a plateau has been reached, or that the Commonwealth has come to an end. Instead he has concentrated on the political and constitutional processes by which nearly thirty British colonies, populated by non-Europeans, became independent, yet retained their membership in a unique international association.

The period which McIntyre covers begins at a date usually associated with the opening of renewed imperial spirit and effort. The book is divided into three parts, with dividing lines at the two dates when the United States entered the world wars, 1917 and 1941. The author's point is that American intervention made possible the Commonwealth's preservation and facilitated its extraordinary transformation. Nevertheless, although American aid and American example were important, the author's own narration makes it quite clear that colonial nationalists were struggling for status and rights which the white dominions had achieved peacefully in the nineteenth century. The turning points were the wars themselves rather than American participation in them. The colonies demanded fulfillment of ideals which Britain itself had spawned and disseminated. Invariably, however, colonial bureaucracies and British governments were one step behind, and so were too late in making the concessions necessary to appease the colonial thirst for autonomy.

Some accounts of the growth of Third World nations have stressed indigenous cultures and pre-conquest resistance movements. McIntyre shows that, though the struggle against colonialism varied greatly from place to place according to local circumstances, it was usually spearheaded by Westernized elites. Mass movements on Western lines were evoked to overcome imperial reluctance and the inertia created by traditional ruling classes that had been induced to accede to, and cooperate with, foreign rule. Some nationalist politicians introduced concepts derived from another Western source, Marxism. At times they also revived latent hostility to foreign rule by recalling traditional folkways. But the revival of historic or tribal cultures was a tool rather than an initiating force.

From a thorough study of published secondary material about the emerging colonies, McIntyre has put together the first comprehensive history of the emergence of the new countries that now make up the Commonwealth. His definition of the present Commonwealth is that it is an association of independent nations, and his objective has been to tell the story of the progress of each from colony to sovereign state. His treatment of events in the original white dominions, and also of the subcontinent after independence, is therefore scant. Reference to the problems of, and the political struggles in, some of the smaller Commonwealth members, such as Cyprus, Malta, and Fiji is also almost entirely lacking. Furthermore, although there are accounts of the Prime Ministers' Conferences, which marked various stages in the development of the Commonwealth, the Secretariat and its significance is largely ignored. Lester Pearson's decisive contribution to its creation is not mentioned. A scholar of the Empire east of Suez, McIntyre tends to look at the Commonwealth from the antipodes rather than from London; there are some traces of earlier antipodean mythology, such as the viewpoint that an effective system of imperial defense based on dominion participation was a practical proposition early in this century. Finally, the maps are merely slight sketches, and many are uninformative. But these are minor criticisms of a book which is a substantial, scholarly work and which has been needed for some time.

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GÉRARD CHALIAND. *Revolution in the Third World: Myths and Prospects*. Foreword by IMMANUEL WALLERSTEIN. New York: Viking Press. 1977. Pp. xxii, 195. \$11.95.

This is a brilliant, if troubling, portrayal of forces which have wracked the Third World in recent decades. It begins with an introductory description of some problems of underdevelopment faced by the Third World and proceeds to descriptions of leading examples of national liberation struggles in Asia, the Middle East, Africa, and Latin America. It closes with an analysis of Marxist-Leninist and Third World concepts of the nature of global problems, with observations on the world economy and the role of the United States as the richest nation.

Along the way, Gérard Chaliand discusses a dizzying array of problems from Marxism to modernization and events from China to Chile, always with attention to social and economic forces and usually with insights which go to the heart of the problems discussed.

Chaliand assumes that a rational ideology is a prerequisite for modernization and notes that only Marxism-Leninism so far has filled this role. He believes that the "clutching at the past of the Arab regimes" and others (p. 109) makes effective modernization impossible, even as it tries to overcome the identity crisis faced by all non-Western peoples, who feel forced to emulate the earlier modernizations of the European powers and the United States. Here, in perhaps his principal oversight, the author neglects the case of Japan, which laid the basis for its impressive postwar performance precisely during the period prior to 1945 when it insisted on a "restoration" of old Japanese values. Perhaps Japan's case is the exception which proves the rule, but the author could have given it more attention, particularly because of the interesting contrast Japan provides with the revolutions of Russia, China, and Vietnam, which properly are a principal concern of the book.

The author states that after the Russian and Chinese Revolutions, "except for Cuba, the only victorious people's war—this one of heroic proportions—has been that in Indochina, Vietnam in particular." Elsewhere he praises the revolutionary war led by Amílcar Cabral in Guinea-Bissau, prior to his assassination in 1973. The paucity of revolutionary successes leads Chaliand to conclude that "the revolutionary potential of the third world had been overestimated" (p. 185). He gloomily predicts that "nothing indicates it is possible to alter the course of a history based on . . . the bondage of the victims and the grief of the vanquished" in the Third World.

Chaliand's European background and leftist orientation lead him to observations which will be unexpected to most Americans, such as the belief that postwar Western prosperity was primarily "thanks to the very low prices they paid for raw materials from the third world" (p. 169). The 1973 quadrupling of oil prices only sought to rectify this inequity for one type of raw material.

Hence the author believes the majority of Third World countries continue to suffer above all from the profit and credit squeeze imposed by neo-imperialism, as well as from overpopulation, and he notes that at least forty percent of the Third World peoples are completely destitute. The hopes of revolutionaries to break the vicious circle of poverty and oppression has proved illusory with few exceptions, because of the errors of leadership, compounded by local conditions and international pressures favoring continuation of the status quo. Local conditions, for example, mean that in most of black Africa (except for Nigeria), in contrast to Asia, the population is too sparse to support effective guerrilla warfare. The role of international capitalism headed by the U.S. was all too clear in

helping to bring about the overthrow of Allende, but in Chile as in Peru and elsewhere, moderate reformers also have either been unable or unwilling to confront inevitable counterrevolution, and hence the central problem, more than the pressures of neoimperialism, has been either "too many reforms or not enough revolution" to deal with the intractable problems of development in the Third World (p. 128).

The author is unable to imagine a means out of the impasse facing most Third World countries, and his pessimism forces him into some rare inconsistencies, for example over the key problem of the necessity for ideology to overcome such dilemmas. Thus, on the one hand he notes its necessity, but elsewhere states there was "too much importance . . . assigned to the possible role of revolutionary ideology," with inadequate attention to local conditions.

Nor is the author more hopeful of the Soviet and Chinese experiments. He sees the emergence of the evils of dictatorial bureaucracy as the inevitable consequence of Lenin's stress on the vanguard party, together with the requirements of running complex societies. As for the United States, it remains the leader of the "world capitalist system" which he regards as the primary cause of the poverty and stagnation of the Third World.

Hence, Chaliand laments, "it is time to announce the death of [the hope for] utopia," whether in Russia, China, or the United States. One is thankful for Chaliand's wide-ranging, perceptive coverage of crucial developments of the contemporary world, if disturbed by his pessimism. The latter places him among the European so-called "new philosophers" or leftist critics of the old order of both Left and Right. This in turn has grown out of existentialist traditions, and one hopes it will not lead to a new nihilism. In any case the book provides a splendid introduction to many of the true problems of our day in refreshing contrast to the elitist analyses that up to now have dominated most U.S. coverage of the non-Western world.

JAMES P. HARRISON
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MARTIN SELIGER. *The Marxist Conception of Ideology: A Critical Essay*. (International Studies.) New York: Cambridge University Press. 1977. Pp. xiii, 229. \$13.95.

Martin Seliger set out to write a critique of Marx's theory of ideology. In those portions of the book which are insightful, Seliger rejects Marx as a relativist (consciousness is totally dependent on

class position) in favor of Mannheim's relationism (consciousness, although conditioned by social class, produces hypotheses which can be tested against reality and emerge as statements of truth).

Three massive flaws exist in Seliger's presentation: 1.) Seliger criticizes a Marx who never existed. Viewing Marx as holding to a copy theory of truth, Seliger was led to interpret the theory of ideology in Marx as a mere reflection of class conditions. In fact, Seliger has criticized Stalinism. While his attack on Stalinism is well founded, Seliger's failure to draw a distinction between Stalin and Marx leads the reader into a gross error: the criticism of Stalinism is also offered as a criticism of Marxism. 2.) Seliger demonstrates an inability to differentiate between concepts. He confuses consciousness and ideology. Interpreting Marx's theory of consciousness as a photocopy of reality, he associates ideology with the duplication of class interests. Seliger fails to distinguish between various aspects of consciousness: as cognition, as rationality, and as a general awareness of culture. He reduces consciousness to cognition, and then simplistically reduces ideology (as a system of beliefs) to the cognition of class environment. Seliger seems unaware of the recent work in Marxist epistemology (Schmidt, Chatelet, Reidel, Bernstein) which has overturned the interpretation that for Marx consciousness was a photoplate (a tradition which Seliger defends) and explores, in the theory of *praxis*, consciousness as active, externalizing, constitutive. 3.) Seliger confused ideology with a Marxist theory of knowledge generally. Ignoring Althusser, he does not isolate ideology from dialectical materialism as a "science" (a constituent part of a theory of knowledge). While Marx did maintain that ideology was expressive of class interests, he additionally held that dialectical materialism was a "science" which had discovered the truth of the evolution of both society and a consciousness which corresponded to that society. Ideologies changed, but dialectical materialism existed independent of class interests and was an instrument for the discovery of truth.

This book is outdated philosophically. Taking Lukacs' *History and Class Consciousness* as the classic definition of a Marxist theory of consciousness, Seliger cut himself off from the innovative contemporary advances in Marxist epistemology. By refusing to confront the insights of Kosik, Rotenstreich, Colletti, Della, Volpe, Petrovic, Stiehler, and others, the author insured that his own work lacked any contemporary relevance to the dialogue within Marxism as it exists at present.

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ZOLTÁN TAR. *The Frankfurt School: The Critical Theories of Max Horkheimer and Theodor W. Adorno*. Foreword by MICHAEL LANDMANN. New York: John Wiley and Sons. 1977. Pp. xx, 243. \$14.95.

The specter of the Frankfurt School haunts sociological enterprise, Zoltán Tar's opening sentence announces. It also haunts his own promising, but severely flawed, book. The aim of filling the purported gap regarding relations between Frankfurt School theory and sociology left by Martin Jay's *Dialectical Imagination* and Gian Enrico Rusconi's *La Teoria Critica della Società* is not realized. For coherent historical accounts of the subject, one still does better taking up these earlier works.

Tar, whose reduction of the Frankfurt School to the "critical theories of Max Horkheimer and Theodor W. Adorno" is unconvincing, offers four main theses. Two—that critical theory qualifies neither as Marxism nor as academic sociology—are neither new nor fruitfully presented. Tar proceeds on the hollow assumption that Marxism is what Kautsky, or Plekhanov, or Zhdanov said (a materialist-positivist science of social laws, infused with a spirit of historical optimism); and that sociology is functionalism. That Marxism, sociology, and the Frankfurt School critique of both are all historical phenomena in permanent crisis in the twentieth century eludes his schematic approach.

Quick to take Horkheimer and Adorno to task for their flights from the empirical, Tar's own relation to facts is not always felicitous. Unfounded assertions, for example, that in the late 1930s the Frankfurt School accepted Dimitrov's definition of fascism (pp. 74–75), appear too often. Nor does he take seriously enough his own admirable emphasis on the biographical matrix of Frankfurt School theorizing. Had he, paradoxically, he might have seen Horkheimer's militant 1930 essays, Horkheimer and Adorno's pessimistic *Dialectic of Enlightenment*, and Adorno *et al.*'s psychological study, *Authoritarian Personality*, as historical components of a unified account of fascism, not merely as contradictory phases in the authors' careers.

Tar's other two theses are more promising, but await fulfillment. Focus on the Jewish element in Horkheimer's and Adorno's work (the Judaic prophetic tradition generally, and the modern German-Jewish, ethically motivated, intellectual Left particularly) is the book's most suggestive moment. Unfortunately, Tar is more intent on debunking the non-Marxist aspects of Horkheimer and Adorno than on grasping their story. His claim that they were finally Jewish neo-Kantian intellectuals on the model of the Marburg philosopher, Hermann Cohen, ignores the central place of Hegel in their work. Worse, he ignores what he might have seen: the drama of Horkheimer and

Adorno's attempt to fuse the Jewish insistence on the primacy of ethics with Hegelian rationalism.

Tar's fourth thesis—Frankfurt School thought is not only a reflection of the “marginal bourgeois-Jewish intelligentsia” but “in the last analysis . . . another existential philosophy” (p. 205)—is stillborn. Here he puts his finger on a rich lode which, if tapped, could illuminate much about the related crises of Marxist and bourgeois thought in this century. Instead, Tar cites Georg Lukács and Ernst Bloch, as if they were impartial observers whose own careers were free of Jewish, marginal, and bourgeois elements, to the effect that the “existentialism” of Horkheimer and Adorno is some sort of offense against reason and the proletariat.

The impulse to pursue the Jewish and existential aspects of Horkheimer's and Adorno's work is sound. Unfortunately, it gets lost in this latest and not most successful effort to lay to rest the ghost of the Frankfurt School.

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F. BÉDARIDA *et al.* *Pour une histoire de la statistique*. Volume 1. Paris: Institut National de la Statistique et des Études Économiques. 1977. Pp. 593. 50 fr.

This volume of thirty-three contributions consists of the papers presented at the June 1976 conference organized by the Institut National de la Statistique et des Études Économiques (INSEE) for statisticians and historians. The book has no internal organization; each contribution is self-contained with full bibliographical data and a summary of the discussion held at the conference. These summaries are short, but quite helpful, since they serve as a critique of the papers and offer the authors an opportunity to clarify or elucidate certain points.

The book deals with the history of the data of statistics, and it focuses primarily on France. Hecht provides a good survey of data collection in Western Civilization to the French Revolution and there is a brief survey of European demographic statistics for the eighteenth and nineteenth centuries (M. Dupaquier), one survey of English developments in the nineteenth century (Bédarida), and one on the German territories before unification (Hoock), but the rest of the volume concerns France during the last two centuries. The contributions on France deal mainly with the availability of sources, although some articles also analyze the data. Topics covered include criminality, education (really literacy), agriculture, industry, railroads, inheritances, medicine, capital, and demography (including one article on the colonies in the seventeenth and eighteenth centuries). Perrot's article on criminality (1780–1830) and Lévy-Leboyer's on land taxes in the late nineteenth

century are particularly good. Lévy-Leboyer shows how the calculation of land taxes, which were used to evaluate capital, flattened the economic curve. His identification of the pitfalls in the use of statistics is valid for all users.

Several articles are concerned with the effect of the political system on the collection of statistics. Does an authoritarian system collect more? This area is explored somewhat for England and France in the eighteenth and nineteenth centuries, but important questions are not asked. Markovitch does not consider why France collected good data only on mining, nor does anyone explore the complete change which takes place in the twentieth century when the more liberal nations (U.S. and England) lead in the collection of statistics rather than, as previously, the more centralized states. Two interesting contributions on the opposition to statistics show that intellectual currents have not changed very much; suspicion of numbers and the use made of them is not unique to modern times.

A noticeable omission in the volume concerns the lack of work on the history of the science of statistics. Bédarida comes closest to discussing this in his survey of England, but we learn little about such men as Pearson or Gossett. It is disappointing that a meeting of statisticians and historians failed to discuss such pertinent problems as the proper use of probability theory, sampling, and factor analysis, or to explore quantification except for a discussion of the development of the current socioprofessional code (CSP) by Desrosières. Demonet's use of factor analysis and Lecuyer's interesting, but rudimentary, use of content analysis demonstrate the need for more cooperation between historians and statisticians.

While one will not find a history of the science of statistics in this book, as the title might imply, there are several good contributions on the history of data collection, especially the one by Desrosières, Mairesse, and Volle on the INSEE and its predecessors. The real value of this volume, however, is for those interested in statistical sources and their intelligent use in the writing of history; for these persons I highly recommend this volume.

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ANCIENT

M. T. W. ARNHEIM. *Aristocracy in Greek Society*. (Aspects of Greek and Roman Life.) New York: Thames and Hudson. 1977. Pp. 221. \$19.95.

Democracy is a Greek word and a Greek invention, associated especially with Periclean Athens. But aristocracy is a Greek concept, too, somewhat

neglected by nonspecialists. This book treats it from Mycenae to Alexander, defining it as a closed, hereditary, cohesive group. To Homer, aristocracy meant having commendable personal qualities, being good at games, and owning land (but not much: Ithaca is a small island [four miles by fourteen], but Penelope had 108 aristocratic suitors.) In its heyday (seventh-sixth centuries B.C.) aristocracy is represented by royal clans at Corinth, first settlers at Cyrene, cavaliers at Colophon, conquerors (of Messenia) at Sparta. M. T. W. Arnheim calls classical Sparta a "republic of demigods," and notes that internal strife destroyed it. In the sixth century popular dictatorships, like Pisistratus' at Athens, sprang up as alternatives to aristocracy. Fifth-century Athens was anti-aristocratic in government, aristocratic in ethos, which implies a belief in natural inequality, like Plato's in the *Republic*. Whether in theory or in practice, Greek society was never egalitarian.

This is an important subject; it is a pity that the author has chosen a polemical tone. Those accused of error include the present and former professors of ancient history at Cambridge, the Wykeham Professor at Oxford, and many others, English, German, and American. Polemic provokes polemic; I prophesy many savage reviews. In my judgment, to assess Athenian aristocracy with anything approaching adequacy one must take into account, as the author fails to do, the sixty-odd clans into which it was divided, including those who could manipulate religion (Eumolpids, Ceryces), and one, the Salaminioi, with a curious invented pedigree, betrayed by an inscription apparently unknown to Arnheim. Another inscription, on the south wall of the Athenian treasury at Delphi, would have enlightened him as to the existence of a *genos* called Eupatridai. Also, he neglects numismatic evidence: the sixth-century Athenian *Wappennünzen*, stamped with what are arguably the armorial bearings of various aristocratic clans. He has an exaggerated idea of the size of Attica, calling "remote" a district only twenty miles from the center of Athens. Finally, his style is unfortunate, abounding with jingles ("degree of agreement," "consolidation of population") and tautologies ("coexisting at the same time," "common masses," "vituperative abuse").

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ALLEN MASON WARD. *Marcus Crassus and the Late Roman Republic*. Columbia: University of Missouri Press. 1977. Pp. vii, 323. \$15.50.

The purpose of this latest work on the political career of Marcus Crassus is to present an itemized

and compact analysis of Crassus' place in the political affairs of the last century of the Roman Republic. The author declares that this study will lead us to a better understanding of Crassus and of his time, because he was much more significant and able than he has been portrayed. There are two reasons why Crassus has been overlooked by the ancient and modern writers. First, there is the question of aristocratic bias against him because he acquired his wealth by way of business activities and not by the traditional method of agriculture or warfare; and second, he suffered a disastrous military defeat at the end of his life, and Roman attitudes toward such a military loss were not very charitable. Thus, Allen M. Ward states that the reader can only really know Crassus through a fresh investigation of his true goals, aspirations, and political activities. In order to accomplish this end, the author makes good use of prosopographic information when written evidence is lacking.

Crassus was born at a time of great turmoil and civil strife in the Roman world. The established methods of achieving political prestige via high office, landed wealth, friendships, family alliances, and intermarriages were no longer secure. In the last century of the Republic two political cliques, the *optimates* and the *populares*, appeared in Roman rhetoric. The *optimates* ("the best men") were those nobles who often opposed substantive change in the face of new conditions, whereas the *populares* were "men who sought to redress the grievances of the social, economic, and political groups outside of the senatorial aristocracy, or at least to play up to their interests for political support in the struggle for offices and honors at Rome . . ." (p. 7). Crassus' family had been firmly established as members of the *optimates* faction. But after the civil wars of Marius and Sulla, in which his father and brother were killed, Crassus was the only surviving adult member of his family. Thus, according to Ward, his two major objectives were to emulate the accomplishments of his father, grandfather, and ancestors and to restore the prestige that his family once held among the *optimates*. To achieve these ends, Crassus amassed power by way of wealth, financial connections, and helping the promising young in need of money. But his methods only brought disapproval from among the *optimates* group.

The revolt of Spartacus was seen by Crassus as an opportunity to achieve military distinction. Yet Pompey received a triumph for the victory, and Crassus had to settle for a simple *ovatio*. This left Crassus still wanting acceptance at Rome and still seeking the restoration of his family's position within the Roman nobility. Thus, as Ward points out, Crassus was forced to join with the increas-

ingly popular Julius Caesar. Crassus worked with Caesar to gain popular support. Ward indicates, however, that Crassus was most reluctant to commit himself to a real *populares* career and that his desire to secure the cooperation and acceptance of the *optimates*, whenever he could, led to what appears to be erratic behavior on his part.

The opus is well done in a most scholarly, thoroughly documented manner. For the general reader of Roman history, the author gives a clear and concise picture of the political groupings in Crassus' age. Ward has also added interesting and useful appendixes for the more specialized reader.

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MICHAEL GRANT. *Jesus: An Historian's Review of the Gospels*. New York: Charles Scribner's Sons. 1977. Pp. 261. \$12.50.

Michael Grant, classical scholar and author of *Herod the Great*, *The Jews in the Roman World* and *Saint Paul*, has now produced a historian's biography of Jesus. In this, the essence of the mission of Jesus is presented as the proclamation and actual ushering in of the Kingdom of God. As a preacher, he sought to effect a change of heart in sinners, and in pursuit of this aim, was prepared to mingle with the outcasts of Jewish society. His character was "stormy": he was capable of displaying "white-hot anger" as well as sentiments of love.

Grant deduces from certain equivocations in the Gospels that Jesus did not regard himself as the Messiah. The titles, he argues, of "Son of Man" (i.e. the *representative* man) and "Son of God" (used in a nonontological sense) express more suitably the view he had of himself.

Repeated emphasis is laid on the fact that, for most of his contemporaries, Jesus was a failure both in Galilee and in Jerusalem. But Grant blames his final condemnation jointly on the Sanhedrin and Pilate (though he fails to take any account of the legal problems raised by the Gospel trial narratives). The tragedy, as he sees it, culminates in the last cry on the cross: in his final agony, Jesus felt himself betrayed by God. Yet his followers' belief in his resurrection, and the fascination exercised by his words, allowed Christianity to live and grow: "the despised . . . Galilean became the Lord of countless millions."

The book is based on a sensible evaluation of the Gospels and a judicious use of secondary literature. The only major issue on which Grant shows no acquaintance with contemporary debate is the Son of Man problem. In the biblical and Judaica domains proper, where he is patently not an expert, there are some errors, but they are without

consequence: Psalms and Odes of Solomon are confused (p. 40); Galilean Aramaic and Hebrew are described as "coarse, guttural dialects," when in fact the opposite was the case (p. 74); chapters 40 to 66 of the Book of Isaiah are assigned to a single Second Isaiah dated to 400 B.C. (p. 241).

In short, this is an enlightened reconstruction of the life of Jesus.

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EUAN W. MACKIE. *Science and Society in Prehistoric Britain*. New York: St. Martin's Press. 1977. Pp. ix, 252. \$22.95.

Euan W. MacKie divides his book into two parts. In part one he gives a detailed account of the work which Alexander Thom has done on the standing stones, stone alignments, stone circles, and other monuments of the Neolithic period of Britain and Brittany, and in part two he studies what he considers to be the implications of Thom's work.

Few who are interested in archeology will not have heard of Thom. Basing his argument on his numerous and very accurate field surveys of a variety of monuments, Thom argues that prehistoric man set them out on the ground using a fixed unit of measurement which he (Thom) has called the *megalithic yard*—IMY=2.72 feet. He has also shown that stone "circles" are not always circles. Some of them are flattened circles, some are ovals, some are ellipses, and so on, and he has claimed that these shapes are deliberate and that their circumferential curves were set out using a geometrical pattern of centers so organized that they gave the desired curves. He has studied the orientations of particular diameters and has argued that they were set out so as to be aligned on the local horizons at points where significant astronomical events took place—risings or settings of the sun on important days such as the solstices and equinoxes and similarly for the moon, though this was not so easy because of the moon's eighteen-year cycle.

Thom has a great and convinced body of followers and Euan MacKie is one of them. MacKie has (if one may use the expression) swallowed Thom hook, line, and sinker! Indeed, he goes on inexorably to apply Thom's principles to other ancient monuments such as the brochs—stone towers built in Scotland at least two thousand years later than the time about which Thom is talking. They too were built according to Thomian geometry and the metrology of the megalithic yard! This despite the severe criticisms to which Thom's work has been subjected by several very reputable mathematically minded archeologists. In Ireland we

have found no evidence for the megalithic yard in a comparable range of neolithic period monuments, though there is some evidence for specific orientations especially to solar events. It has also been shown that Thom's geometry is not necessary to achieve the circle shapes. Different patterns of a few pegs stuck in the ground around which a rope is pulled will give most of them.

In part two, MacKie uses Thom and evidence from various other sources—mainly from recent surveys and excavations—to support his theory that Neolithic Britain (with which he would no doubt include Ireland also) was run by a skilled class, “groups of wise men—perhaps a theocratic élite—[who] lived a life apart in special ceremonial centres or monasteries which can be identified by their unusual nature and also by specific evidence from artefacts and food refuse” (p. 208). Every kind of “evidence” from all sorts of disparate sources is used by MacKie to support this theory (he even drags in the Maya of Central America), and all awkward questions are slid over in a masterly fashion. The book should have a big sale among those who like their archeology to be highly speculative.

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MEDIEVAL

HARRY A. MISKIMIN *et al.*, editors. *The Medieval City*. New Haven, Conn.: Yale University Press. 1977. Pp. x, 345. \$18.50.

This is a study in honor of Robert S. Lopez and, as with most *Festschriften*, the quality, breadth, and size of the contributions vary considerably. As such works seldom have a single theme, it is the reviewer's first duty to present the contents, even if this does not allow for a display of his own intellectual virtuosity. This book contains: David Herlihy, “Family and Property in Renaissance Florence”; Bariša Krekić, “Four Florentine Commercial Companies in Dubrovnik (Ragusa) in the First Half of the Fourteenth Century”; Frederic C. Lane, “The First Infidelities of the Venetian Lire”; Carlo M. Cipolla, “A Plague Doctor”; Benjamin Z. Kedar, “The Genoese Notaries of 1382: The Anatomy of an Urban Occupational Group”; Diane Owen Hughes, “Kinsmen and Neighbors in Medieval Genoa”; Edward Peters, “*Pars, Parte*: Dante and an Urban Contribution to Political Thought”; A. L. Udovitch, “A Tale of Two Cities: Commercial Relations between Cairo and Alexandria During the Second Half of the Eleventh Century”; S. D. Goitein, “A Mansion in Fustat: A

Twelfth-Century Description of a Domestic Compound in the Ancient Capital of Egypt”; Joshua Prawyer, “Crusader Cities”; Harry A. Miskimin, “The Legacies of London: 1259–1330”; John Munro, “Industrial Protectionism in Medieval Flanders: Urban or National?”; Joseph R. Strayer, “The Costs and Profits of War: The Anglo-French Conflict of 1294–1303”; Richard C. Hoffmann, “Wrocław Citizens as Rural Landholders”; and Sidney Cohen, “The Earliest Scandinavian Towns.”

The remainder of this brief review will deal with a few of these articles, selected somewhat arbitrarily according to this reviewer's interests. Udovitch's article is a delightful piece, drawing upon his own translation of four letters from the famous Cairo Geniza. He shows how the evidence of “informal” business relations reveals “that the economic relationship between the merchants . . . was an extension of their personal relationships. There was no clear line between personal and commercial concerns. . . .” This article, like that on Prawyer, offers a fine introduction for the non-Islamicist. Prawyer's fascinating contribution demonstrates clearly how the Crusader cities came to be characterized by a juxtaposition and not a symbiosis of disparate elements, whether topographic, institutional, or cultural, derived from the various “Jewish, Hellenistic, Byzantine, and Moslem” and the diverse Western European peoples who lived in Palestine.

Strayer offers a thought-provoking analysis of war expenses and their effects on the various sectors of French and English economy and society. Demonstrating that wages constituted the greatest expense, he examines the wages of various types of fighting men and compares them with their peacetime incomes. He concludes that “war transferred wealth from the productive to the non-productive sector, from the savers to the wasters, from the innovators to the traditionalists.”

All who teach medieval urban history deal with the problem of town origins. Using largely archeological evidence, Cohen confronts the issue squarely. Laying much weight on the absence of walls, he argues forcefully that “it is reasonably certain that the [earliest Scandinavian] towns had an indigenous origin and attracted foreign visitors later.”

Turning to Italian history: Lane presents a worthwhile if demanding and complex study of the nature and history of the Venetian shift “from a silver standard to the use of a gold standard in large monetary transactions.” Herlihy's article, strongly buttressed by his study of the Florentine *catasto* of 1427, is an intriguing exploration of inter-relationships between economic, social, and cultural phenomena. He finds that “the richest one

hundred Florentine households alone controlled nearly twenty percent of all the social capital of Tuscany." There follow interesting discussions of the role played by war in the recycling of wealth and of Tuscan sharecropping and the patron-client relationships it encouraged. The second half of the article examines how affluent families tried to manage their properties; it includes a fascinating analysis of the treatment of their offspring by patrician families and an explanation of how the parental strategy of gambling on their offspring "generated at Florence distinctive patterns in the distribution of wealth, according to the age of its owners." Here Herlihy also contrasts urban with rural families.

This useful volume contains a bibliography of Lopez's writings, an index, and footnotes rather than endnotes. The press skimmed by printing the notes in too small a font and in all but eliminating the margin at the bottom of the pages. A scholar of Robert Lopez's stature deserves better. Nonetheless the lines are justified, and not every Ivy League press can still make that proud boast.

WILLIAM M. BOWSKY
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Davis

ROBERT M. GRANT. *Early Christianity and Society: Seven Studies*. San Francisco: Harper and Row. 1977. Pp. xii, 221. \$10.00.

Books seem to flow with surprising frequency from the pen of the University of Chicago Divinity School's distinguished church historian, Robert M. Grant. This volume consists of seven independent studies, two of which were previously presented as lectures at universities. In "The Christian Population of the Roman Empire," the author makes educated guesses concerning the number of Christians in certain cities at certain times (e.g., 15,000 or 20,000 in Rome in the middle of the third century) and examines the social status of Christians (Christianity was not a proletarian mass movement). Chapter two, "Christian Devotion to the Monarchy," argues that Christian attitudes to the Roman state ranged from support to bitter hostility, but the state was "the sphere of the Church's life," and in it the Church gradually grew up as a state within the state. This was because of the Pauline idea of the church as a body (1 Cor. 12) and the institution of the church court. "Taxation and Exemption" describes how increased taxation resulted in demands for tax exemption from which pagan priests and, later, Christian clerics benefited. The result was avarice and avoidance of public responsibilities. According to chapter four, "Work and Occupations," it was gnostics, not Christians, who "resolutely re-

sisted the meaningfulness of work" (p. 68), but the Church disapproved of several occupations. Often Christian theologians had a low regard for poor people, whom they considered to have bad characters; there was no attempt by Christians to emancipate slaves. Grant states in "Private Property" that there was "little enthusiasm for socializing of private property" (p. 104) and that Christian theologians mostly defended it with the argument that possession of property depended on the use made of it. In the later years of the fourth century many fathers criticized the excessive accumulation of property in the hands of wealthy persons, as graft and corruption spread in the empire. Chapter six is "The Organization of Alms." Almsgiving gradually developed into gifts for Church purposes intended to be spent for the support and burial of the poor, and the needs of orphans and prisoners. Monthly payments to ministers were common, at least from the third century. The Roman distribution of grain to the populace was entrusted to the Church by Constantine and this gave rise to abuses by Church authorities. The final chapter, "Temples, Churches, and Endowments," discusses the first Christian churches (basilicas) built before Constantine's time; after the gradual suppression of paganism many pagan temples and endowments were taken over by the Christians.

The publisher has advertised this book as "seven breakthrough studies that challenge historical assumptions and demythologize an era. . . ." Apart from the fact that the work of "demythologizing" has progressed since Harnack, the book is much better characterized by the more modest statement of the author himself in the preface: this is a book for people whose interest in early Christianity "is not strictly academic" (p. vii). To be sure, there is nothing wrong with popular books so long as they are based on sound scholarship and research; in this respect Grant can justly claim competence. Yet it must be questioned whether these seven major problems can be adequately dealt with in 163 pages, many of which are taken up by reviews of the opinions of early Christian authors from St. Paul to St. John Chrysostom. These reviews often read like term papers, leaving little room for analysis; of course, four centuries is much too long a span of time to be covered in detail. The result is a survey from which experts in ancient and church history will learn little but which, however, will be read with profit by anyone else who is interested in the field.

STEPHEN BENKO
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Fresno

RAINER CHRISTOPH SCHWINGES. *Kreuzzugsideologie und Toleranz: Studien zu Wilhelm von Tyrus*. (Mono-

graphien zur Geschichte des Mittelalters, number 15.) Stuttgart: Anton Hiersemann. 1977. Pp. vi, 329. DM 145.

Rainer Christoph Schwinges' work, originally his doctoral dissertation and now a worthy member of a distinguished series, presents William of Tyre against a background of crusading ideology as a pioneer of tolerance toward Islam and Muslims in theological, biographical, and legal theory and practice. His primary source is William's *History of Deeds Done beyond the Sea* (his *History of the Deeds of Oriental Princes* being lost); his methodology is a comparative, qualitative interpretation of attitudes, opinions, ideas, judgments, prejudices, and their interrelations.

The book has four approximately equal parts. Part one briefly describes William's life and works, places him with respect to both crusading activity and present scholarship, and defines tolerance. Part two surveys the generally tolerant views of Oriental Christians and the ideological interpretations of Byzantines and Latins regarding Islam and Muslims, then shows how William built upon the former but surpassed all both in his knowledge of Islam and Islamdom and in his recognition of Islam as an autonomous religion. Part three examines William's portrayals of past and contemporary Islamic leaders. For him they were individuals, neither part of an undifferentiated mass of religious foes nor stereotypes. In contrast to Augustinian and poetic views of Muslims, William set aside religious motives and judged individuals according to rational/natural norms and individual qualities, admitting some (e.g., Nūr ad-Dīn) to the City of God.

The fourth part (chapters four and five of part three) presents William's legal and missionary thinking. He held that *injuria* (the ground for a just war) could be suffered by both parties to a dispute, hence both could be fighting a just war. He further argued that Palestine was now the *patria* of both Muslims and transplanted Westerners, and both had a right to *libertas* in home, family, property, and religion. Extending that equality before the law to diplomatic relations, he upheld the sanctity of pacts with Muslims against the standard Christian view that pacts with non-Christians were not binding. In thus stressing the principle of subjectivity in law, William went beyond contemporary legal opinion, anticipated Vitoria, and hence merits an outstanding place in medieval international law. As archbishop he treated Tyrian Muslims fairly and limited missionary activity to discussions with Muslim inquirers.

To highlight William's uniqueness Schwinges uses two comparative processes. He sets William's ideas and statements alongside those of contempo-

rary writers and workers in the German crusade against the Slavs (so emphasizing how Christian thought about Muslims was ideologically similar to that regarding other "heathens"). And he details how the Old French translation of William's *History*—*L'Estoire de Eracles*—regularly omits, modifies, or falsifies those passages where William's innovations appear (so showing ideology and innovation in confrontation).

In ample notes Schwinges takes positions on disputed points, while his full bibliography includes important works not often cited, notably unpublished German dissertations. By placing William in the forefront of medieval tolerance, counterpointing his position with those of Western poets and chroniclers, and elucidating many smaller points (what tolerance was, why Muslims were termed pagans and heretics, how Augustinian thought-forms shaped Christian attitudes toward Muslims), Schwinges significantly advances our understanding of both William and medieval Christian-Muslim relations.

JAMES WALTZ
Eastern Michigan University

HORST ZETTEL. *Das Bild der Normannen und der Normanneneinfälle in westfränkischen, ostfränkischen und angelsächsischen Quellen des 8. bis 11. Jahrhunderts*. Munich: Wilhelm Fink Verlag. 1977. Pp. 333. DM 78.

True to its title this work gives a many-sided portrayal of the Vikings as seen by their enemies. Each part of this Frankish and Anglo-Saxon picture is methodically catalogued and discussed, beginning with the ethnic divisions among the Northmen, the geography of their homelands, their domestic politics, economy, culture, and their "character." The longest section traces in the Continental and English sources parallel phases and caesuras of the invasions, various motives given to account for the attacks, the organization, leadership, and numbers of the northern invaders, Scandinavian tactics and weapons, and the immediate effects of the invasions. The work concludes with a section on the Danelaw and Normandy with emphasis upon parallels in diplomatic negotiations, Viking leadership, and integration of the Scandinavian settlers with native inhabitants.

This examination of contemporary Frankish and Anglo-Saxon sources has self-evident value, particularly because it has not been done so comprehensively before. A slight tendency to interpret some of this evidence superficially (e.g., p. 128), to belabor some obvious points (e.g., p. 256), and to include in the text boring lists of all works which support the same detail (e.g., p. 41) is outweighed by the vivid sense of immediacy that is afforded by Horst Zettel's close study of the many and varied

sources. The major disappointment is Zettel's apparently unquestioning faith in the accuracy and objectivity of the Frankish and Anglo-Saxon evidence and his denunciation of that from Scandinavia because of its late date of composition and lack of objectivity. Surely the truth about the Vikings cannot be found entirely within the pages of the chronicles, annals, and saints' lives written by their enemies, and surely the later Scandinavian descriptions of Viking activities cannot be dismissed entirely as romanticized fiction because they were written several centuries after the events. It may well be, in fact, that the Scandinavian evidence is the more objective in some respects for the very reason that it was written long after the Viking era. In any case some of the important Scandinavian sources are not so far removed from the era as Zettel seems to believe, for certain poems of the Edda and skalds may well have achieved definitive form centuries before being committed to writing. Zettel's lack of respect for the Scandinavian sources makes impossible their comparison with those of the Continent and England, a lost opportunity that decreases the value of the book.

Somewhat illogically Zettel does not shrink from using modern historians who are not as suspicious of the Scandinavian sources as he is. But these historians seem useful to Zettel only to corroborate the Frankish and English evidence, not to correct or augment it, for if they do not agree with this evidence *in toto* or if they should have the impertinence to disagree with it, Zettel's loyalty to the Frankish and English scribes never wavers. Two examples will suffice. First a minor one: Johannes Brønsted, in his widely-known book *The Vikings*, says that wine was a drink favored by the rich and well-born in Viking society. Zettel says that wine indeed was well liked by Vikings, and he cites several Continental and English sources to support the point; but, he says, there is no evidence that it was favored by an "upper class." He ignores therefore the evidence of *Rigspula*, part of the poetic Edda, the obvious source for Brønsted's statement. Zettel also says there is no proof that Viking marauders sometimes doubled as merchants even though this statement is erroneously repeated, as he says, by most of the modern literature. This view not only ignores the abundant testimony of sagas and skaldic poetry, but even possibly that of the *Anglo-Saxon Chronicle* (see *sub anno* 787, ed. Garmonsway [1953]; and *ibid.*, pp. 54-55, n. 2).

In sum, this work is usually interesting and in some ways valuable, but it must be regarded as no more objective than the sources upon which it is based.

BRUCE E. GELSINGER
San José State University

KURT-ULRICH JÄSCHKE. *Wilhelm der Eroberer: Sein doppelter Herrschaftsantritt im Jahre 1066*. (Konstanzer Arbeitskreis für mittelalterliche Geschichte, Vorträge und Forschungen, Sonderband 24.) Sigmaringen: Jan Thorbecke Verlag, 1977. Pp. 112. DM 38.

The *Carmen de Hastingae proelio* has usually been treated as the least valuable of the early narratives of the Norman conquest of England. In recent years Sten Körner, Frank Barlow, and the *Carmen's* editors Catherine Morton and Hope Muntz have argued that it ought to be esteemed more highly. Among other things, the editors have shown that it was composed in 1067 and is therefore the earliest of all the accounts of the conquest. In the work here under review Kurt-Ulrich Jäschke follows these writers' re-evaluation of the *Carmen*, adds further arguments to vindicate the poem's historical reliability, and then goes on to draw out the consequences of the better estimate of the *Carmen* in a startling reassessment of how the victors of 1066 viewed Duke William's succession to the English kingship. As usually understood, the line taken by the conquerors was that William was King Edward's rightful successor and that by destroying the usurper Harold and being crowned at Westminster on Christmas Day he became King Edward's successor in fact. But Jäschke observes that the *Carmen*, which tells the story from the invaders' viewpoint, tells it distinctly otherwise. It portrays Harold as truly a king, though wrongfully so. Killed in the battle, he is buried as a king on a hill by Hastings, where in death he may guard the port. And from the funeral on the hill William of Normandy departs as king, always so called from that point on in the *Carmen*. In the subsequent ceremony at Westminster he was also "made king" in some other unspecified sense. So he entered twice upon the kingship. On the hill by Hastings at least, if not also at Westminster, he clearly succeeded to King Harold rather than King Edward.

The evidence of the *Carmen* is impressive. Jäschke makes it much more so by finding support for it in circumstances and analogies. He explores the diversities of political thought that could occasion such a double accession as William's. He shows that other English kings had reigned before their coronations. He adduces other cases of royal burials like Harold's and royal successions like William's on the hill by Hastings. He studies the English coins of Harold's time and of William's to demonstrate that Harold's kingship was effective throughout England and that William so far acknowledged Harold's royalty as to follow some of his innovations in coinage. He argues that in his coronation at Westminster William was not hark-

ing back to King Edward but imitating King Harold, the first to have been crowned in the abbey.

Jäschke's scholarship is excellent and his contribution important. I did not expect to be convinced, but I am.

DONALD W. SUTHERLAND
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NANCY F. PARTNER. *Serious Entertainments: The Writing of History in Twelfth-Century England*. Chicago: University of Chicago Press. 1977. Pp. xi, 289. \$18.00.

The twelfth century was the first great century of English historical writing, perhaps the greatest before the eighteenth, and Nancy F. Partner's *Serious Entertainments* is the second book-length essay on it. Its predecessor, Heinz Richter's *Englische Geschichtsschreiber des 12. Jahrhunderts*, appeared in 1938, and for Anglo-American medievalists was all but lost in the war. Like Partner, Richter concerned himself with the authorial personality of three important chroniclers. For the rest, the two volumes are as far apart as *Ideengeschichte* and the eclectic tradition of Anglo-American medieval studies. Partner's book is much the more successful, mainly because she makes her case through penetrating *explication de texte*.

Serious Entertainments is, as the acknowledgments say, "an exploration of style of thought and personality and expression." Its method combines historical commentary and literary criticism—more adequately, I believe, than any other publication on medieval historiography since Helmut Beumann's essays on Widukind of Corvey and Einhard. Six remarkable chapters examine three chroniclers about whom little has been written: Henry of Huntingdon (d. ca. 1154), William of Newburgh (d. ca. 1201), and Richard of Devizes (d. ca. 1202). With a keen sense for the interplay of literary convention and personal sentiment, Partner presents Henry as a skillful practitioner of *contemptus mundi* historiography, able to state genuine "mistrust of the world's empty promises" through *ubi sunt* commonplaces; William as a person of large and erudite vision who kept his head even when confronted with the oddest things, like vampires; and Richard as the historian-satirist, "marvelously funny about Carthusians," whose *Chronicle* "gives us a teasing entrance into an earth-bound society of only temporal dimensions, where anything can be counted or calculated by some finite measure, and that which surprises does not, in the end, signify anything." Discerning translations accent the exposition, and soundness of scholarly judgment gives these chapters authority.

Three final chapters bring the three historians

together in a broad discussion of certain problems which, to modern readers, make medieval historiography seem so strange. The first is historical evidence: how were medieval historians equipped to establish it? Then, literary form: why do chronicles seem to be "just one damn thing after another" (p. 194), that is, shapeless? Finally, Christian ideology: when did it impinge on the writing of history? As essays on the peculiarity of medieval historiography, these chapters come much closer to the truth than did William Brandt's *The Shape of Medieval History*. Yet they are less satisfying than the first six, mainly because they attempt so much without the aid of a fairly large body of relevant scholarly literature. The discussion of literary form is the best of these chapters, though the unconvincing and repeated claim that vernacular narratives were "the natural contemporary models for history" (p. 196) detracts from the otherwise helpful pages on the conventions that prevailed alike in fictional and historical works. Partner leaves the impression that if historians did not imitate vernacular narrators, then they had no guidance at all (p. 195). I am confident that works like Richard's *Chronicle* would have been exactly the same if the fictional narratives had never been written.

In any case, this is far more than a promising first book. Everyone interested in Henry, William, and Richard should henceforth begin with it, not with Antonia Gransden's *Historical Writing in England, ca. 550 to ca. 1307*. The six chapters on these authors are models for the study of other chroniclers. *Serious Entertainments* will in fact repay non-specialist readers, for it moves on waves of the most diverting, often sparkling, language.

ROGER RAY
University of Toledo

BRIAN K. ROBERTS. *Rural Settlement in Britain*. (Studies in Historical Geography.) Hamden, Conn.: Archon Books. 1977. Pp. 221. \$12.50.

Over the last twenty-five years medieval archeology and aerial photography have, in their different but complementary ways, revolutionized our knowledge of the origins and evolution of rural settlements in long-settled parts of Western Europe. Nowhere is this more true than in England. Brian K. Roberts' book, published as a study in historical geography, is a welcome and timely survey of recent interdisciplinary work on aspects of the complex relationship between man and the land under changing economic and social conditions.

His treatment is wide ranging; it begins with two chapters on settlement and vegetational changes in prehistoric and Romano-British times,

both periods in which pollen analysis and radiocarbon dating have radically altered earlier interpretations. Of necessity these two chapters are very general, and unfortunately they do not include any plans of field monuments or excavated sites. Roberts then moves on to discuss the evidence of the Domesday survey of 1086, and looks retrospectively at the Anglo-Saxon and Scandinavian phases of settlement and at the current debate over the degree of continuity between the Late Iron Age, Romano-British, and Anglo-Saxon periods. The fourth chapter is mainly concerned with colonization and expansion in the later Middle Ages and several useful models are introduced when "thinking about origins." Two final chapters focus on the past and present morphology of villages, hamlets, and farms. Not surprisingly, the author draws heavily upon his own detailed studies of County Durham and those of others in Yorkshire; if northeast England is perhaps over-represented in this book it deserves to be. Conversely, there are few mentions of Scotland and Wales, in part a reflection of the lower density of rural population in upland Britain in historic times but also of the relative lack of new work in these areas.

Unfortunately, the format of this book is poorly suited to its content. The page size is too small for the reproduction of detailed maps and diagrams, most of which have already been published elsewhere and are overreduced for inclusion here. Aerial photographs (eleven obliques are included) have not reproduced well. In a book where illustrations are often crucial to the arguments these are serious drawbacks. Moreover, if there is a good reason for cream colored paper it is not apparent; the book has a somewhat dated appearance and the publishers have not done the author justice. Everything looks cramped; the involvement of the Shoe String Press seems apposite.

In short, the content of this book is far better than its appearance would suggest. Roberts' survey is scholarly and well documented. His book is essential for an up-to-date view of work in this field in England, and it is to be hoped that Continental scholars might produce similar overviews of recent work in their countries. But hurry before many of the important sites disappear; as Roberts stresses, bulldozers wait for very few men.

ROBIN E. GLASSCOCK
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SUSAN REYNOLDS. *An Introduction to the History of English Medieval Towns*. New York: Oxford University Press. 1977. Pp. xiii, 234. \$17.50.

Susan Reynolds calls her book an introduction, and it achieves splendidly its aim of opening up the

subject, summing up most fairly the aspects of medieval English urban history which have received attention in the past, showing the several fronts on which scholars are now working, and suggesting lines for future investigation.

The whole medieval period is covered; the first two chapters cover the Anglo-Saxon settlement and the centuries which saw the origins of growth, to the Norman Conquest, but the bulk of the book is concerned with the period after 1100, with chapters entitled "The Twelfth and Thirteenth Centuries," "Urban Society before c. 1300," "The Growth of Independence," "Town Government and Politics in the Twelfth and Thirteenth Centuries," and "Demographic and Economic Change" and "Social and Political Change" in the later Middle Ages. The work ends, after "A Postscript on Topography," with a useful glossary and a most efficiently arranged and numbered list of works cited, to which the footnotes throughout the book are linked, an excellent system which makes for certain identification without repetition.

Anyone coming newly to the study of English urban history will be saved much frustration by the firm guidance this book offers, and those to whom urban history needs no real introduction will welcome having the subject exposed with such vigorous clarity. So many aspects of town history have been rendered obscure by writers obsessed with local minutiae, or overcome by indecisiveness amid conflicting theories, or simply uncritical of anachronistic terminology, that a book as boldly clear as this is indeed most valuable.

The author defines a town as "a permanent human settlement . . . with a significant proportion (but not necessarily a majority) of its population living off trade, industry, administration, and other nonagricultural occupations, . . . the town forming a social unit more or less distinct from the surrounding countryside." Taking towns as her subject has freed her from the restrictive consequences of concentrating only on boroughs, but she has not shied away from delving into the definition of the term borough, along with other controversial terms like commune, guild, and partriate.

If it is the test of a good introduction to leave the reader eager to plunge further into the subject, and to provide bibliographical guidance to this end, this book scores highly. Reynolds describes her work as an attempt to survey the present position of studies, and there is clearly a very wide familiarity with source material of different types, along with a tremendous wealth of reading of interpretative works, behind this skillful synthesizing.

What this book contributes to the present study of urban history is clarity and perspective. By economy of style, it packs a great deal into a

little—eleven hundred years of English urban history, with no small consideration of Continental developments—and it reads with pace, and a touch of humor.

HELEN M. JEWELL
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BARBARA HARVEY. *Westminster Abbey and Its Estates in the Middle Ages*. Oxford: Clarendon Press. 1977. Pp. xii, 499. \$34.50.

This is a subtle, original, and important book on the relations between landlordship and tenantry in medieval England, of much wider application than its rigorously concentrated focus on Westminster Abbey might at first suggest. The book is not a study of the economic development of the Abbey's estates in the usual form and has nothing or next to nothing to say about its administrative organization or the nature and quality of its religious and cultural tradition (perhaps suprisingly, the Abbey produced no scholar or thinker even approaching first- or second-line stature except Abbot Gilbert Crispin, ca. 1095–ca. 1118). Rather, it takes a fresh and valuable look (easier to comprehend and appreciate in the reading than to capture or articulate in a brief review) at the social and legal forces making for (and inhibiting) change, that informs us as much of the tenants' point of view and interests as those of their landlords. Barbara Harvey has carefully balanced, and distinguished, patterns that were *sui generis* and patterns that conformed to wider national movements, and she has presented them with as much statistical precision as the sources will allow. The evidence thus confirms and reinforces certain wider features, with some significant variations. What gives the book distinction and special importance is the double focus—tenants no less than lords—that informs and controls its structure.

Westminster Abbey occupied, of course, a special place in English national life, as the coronation and (later and less consistently) the burial church of kings. Not surprisingly, the wealth, importance, and sheer size of the monastic community often turned on strategic instances of royal generosity and patronage, notably those of Henry III, Henry V, and Henry VII. Yet royal support need not always have been an unmixed financial blessing, for in outshining it perhaps also hindered other sources of beneficence. At bottom, the Abbey's prosperity depended on its own priorities and policies. Twelfth-century alienations, undertaken for other than economic reasons but with adverse fiscal consequences, were offset in the thirteenth and earlier fourteenth centuries by a rigorous policy of resumptions, purchases, and in-

sistence on dues and services, on which the effects of such legislation as *Quia Emptores* and *Mortmain* were marginal. A conservative and tenacious adherence to such policies, more instinctive than rationally calculated, marked the entire fourteenth century, and in consequence the social changes of the postplague era were effectively delayed until the fifteenth century: the decline of villeinage, the rise of a peasant land market of any major significance, and the winning through to partibility of tenants' holdings.

This sort of book does not make for easy or rapid reading, but its plan is intelligent and successful, and its rewards the greater for that. Two demurrers might be entered. The discussion of fifteenth-century demesne leasing somewhat awkwardly precedes the invaluable chapter on thirteenth-century purchases. Some chapters or portions of chapters are based on data largely confined to the elaborate appendixes, with the result that what is gained by clarity of discussion is partially offset by difficulties in correlating general statements in the text with their evidence, many pages removed. But no organizational scheme for a work of such intricate design can be foolproof. Harvey has thought harder, longer, and more fruitfully about the estates of Westminster Abbey than—at times—the monks and abbots did themselves; the result is a model study of the rate and direction of change, both short- and long-term, in a traditional society. This twin achievement must be the final impression of every scholar reading and profiting from her book.

MICHAEL ALTSCHUL
Case Western Reserve University

N. A. KHACHATURIAN. *Vozniknovenie General'nykh shtatov vo Frantsii* [The Origin of the Estates General in France.] Summary in French. Moscow: Izdatel'stvo Moskovskogo universiteta. 1976. Pp. 207. 1 r. 31 k.

The intent of the author of this monograph seems to have been to place the development of the Estates General in early fourteenth-century France in the context of the Marxist theory of class struggle and a Marxist model of state development from feudal to limited monarchy. Although the work is competently and honestly done, N. A. Khachaturian does not succeed in achieving her aim, owing to the fact that the material does not fit the preconceived context. This is tacitly admitted by the author as she points out the manner in which the crown gerrymandered the selection of representatives and used the calling of the Estates General to facilitate the growth of the power of the crown. The author is forced to argue that the early

history of the Estates General in France is typical of the development from feudal to representative government in Western Europe, while also arguing that, owing to the concrete historical circumstances in which the earliest meetings of the Estates General were summoned, the representative institution in fourteenth-century France was not as effective as Parliament in England or the *Cortes* in Spain, and that royal policy as manifested in the use of the Estates General in the early fourteenth century was a continuation of policies developed in the thirteenth century.

In sum, I do not think that many specialists will find this book an essential item. It is interesting, however, as an example of the almost insoluble problems involved in trying to impose a rigidly preconceived theory on specific historical problems.

DAVID HARRY MILLER
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WERNER PARAVICINI. *Guy de Brimeu: Der burgundische Staat und seine adlige Führungsschicht unter Karl dem Kühnen*. (Pariser Historische Studien, number 12.) Bonn: Ludwig Röhrscheid Verlag, 1975. Pp. 807.

The important part played by Guy de Brimeu, lord of Humbercourt, in the Burgundian state system under Duke Charles the Bold was appreciated by those citizens of Ghent who had him decapitated on April 3, 1477, three months after Charles' death on the battlefield before Nancy. A youthful companion of the duke, Guy also represented the third generation of his family in Burgundian service, a connection that had rescued this line of Picard barons from the late medieval nobility's permanent peril of depression or extinction. Court offices, administrative positions, ducal honors (three Brimeus were charter members of the Order of the Golden Fleece)—all these rewards had raised the family's estate and prepared the way for Guy's spectacular rise in the decade of Charles' rule. Named ducal lieutenant in the principality of Liège, he played a moderating but still active part in the suppression of rebellions there and in the final conquest and devastation of the city. He then integrated the Liège lieutenancy into a broader one comprising all the Burgundian lands along the Meuse, with a central governmental council in Maastricht.

Werner Paravicini devotes most of his monograph to tracing these activities in overwhelming detail. At the same time he looks to the wider framework since Brimeu's important share in the centralization and homogenization of Charles the Bold's state was not limited to his Meuse lieutenancies but involved his work as permanent ducal

councillor and other public acts in the central government (see esp. p. 395). While the book does not give a systematic account of Brimeu's work at this level, the references are there, and a magnificent "Itinerary" will allow others to take up the task. In any case Paravicini shows that Brimeu's Meuse lieutenancy was not a "state within a state" (cf. R. Vaughan, *Charles the Bold* [1973], p. 254) but represented the sort of coincidence of interests—the prince's and his agent's—that underlay most governmental development in the later Middle Ages.

Paravicini's interest in such basic questions of late medieval political sociology both informs and balances the detailed work. The tedious but essential three hundred pages on Brimeu's work in Liège and the other Meuse lands make possible some very interesting chapters on Brimeu's economic and sociopolitical position, his relationship to the ducal person and power, and finally the subject Paravicini lays out as a task for future study: "the court nobility in the state of the dukes of Burgundy." The present monograph is indeed conceived as a case study in this sense, with Guy de Brimeu's own story framed both by a history of the family before and after him and a constant play of references to contemporary analogues. We see the Picard aristocracy attracted to Burgundian service well before Picardy had entered the ducal conglomerate; that Picards formed the largest single group among the Knights of the Golden Fleece from 1430 to 1477 can be taken as a sign of their importance to the dukes. The rewards of service brought such families into the prime sphere of Burgundian power, the Low Countries, and in several cases the Picard connection was eventually left behind. Guy de Brimeu's annual revenues from his family lands in the mid-1470s amounted to only about 1,000 *livres tournois*—a mere twentieth of his total revenues; the balance consisted of 11,500 *l.t.* from confiscated lands assigned to him in both the Low Countries and neighboring parts of France, 5,000 *l.t.* in gifts and pensions from those seeking his favor as ducal lieutenant, and 5,500 *l.t.* in salaries for his various offices (pp. 421–24).

Guy and his family can thus be taken as exemplars of the general movement, from local baronial status in one or another region to Burgundian court nobility and thence to the high nobility of the Low Countries. If we had a series of detailed works like this we could deepen and extend such perceptions into a more solid understanding of the dialectic between nobility and princely government that kept Europe aristocratic so long after the bourgeoisie had begun its famous rise.

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DUANE J. OSHEIM. *An Italian Lordship: The Bishopric of Lucca in the Late Middle Ages*. (Publications of Center for Medieval and Renaissance Studies, Los Angeles, number 11.) Berkeley and Los Angeles: University of California Press. 1977. Pp. xvi, 211. \$9.00.

This monograph is the first major study devoted to the temporal lordship of an Italian bishopric in the later Middle Ages. Episcopal temporal lordships have indeed been studied previously, by Volpe, Violante, Jones, and Herlihy, among others, but only tangentially as part of other and wider issues such as Church reform, diplomacy, and the growth of the communes. By limiting his focus to the bishopric of Lucca as a temporal lordship, Duane J. Osheim attempts to gather together these variegated approaches into a total picture of change in episcopal life from the eleventh to the fourteenth centuries.

Osheim analyzes a broad sweep of topics concerning the bishopric: the building up of possessions, the administration of those possessions, the secular jurisdiction of the bishopric over certain parts of the diocese (e.g., the *jura*), the relationship with the commune of Lucca, and the sources of episcopal wealth. Finally, he compares these aspects of Luccan episcopal life with other Tuscan bishoprics. The investigative theme that serves as a connecting thread among these topics is the transformation from civil power to rural lordship experienced by the Luccan and other Italian bishoprics in the late Middle Ages.

Departing from an older historiographic framework of dialectic between bishop and emerging commune, Osheim concludes that the bishopric's relationship with the commune of Lucca evolved by the thirteenth century into an "uneasy" partnership, broken only briefly in 1307, 1320, and 1350–51 by uncharacteristic major crises. Indeed, most of the conflicts that occurred were with the rural communes, not with Lucca. According to Osheim, the roots of this cooperation go back to the eleventh and twelfth centuries: at that time religious reforms inspired new foci of piety—the cathedral chapter, the *pievi*, monasteries, and hospitals throughout the diocese—all of which competed with the bishopric for pious donations. The bishopric responded by attempting to regain lost lands and jurisdictions and by reorganizing these possessions into vast estates intended to supply expanding urban markets. The resulting ruralization of the bishopric's temporalities and the consequent removal of the bishop's interests from the city effectively removed the basis of potential bishop-commune conflict. A further and later check on episcopal claims to civil power was the successful challenge of the cathedral canons to the

bishop's control of the diocese and the passing of control of ecclesiastical rural estates to laymen in the second half of the thirteenth century. Both economic change and religious reform thus contributed to the transformation of the bishopric from a civil power to rural lordship.

Osheim carefully sets his thesis against other scholarly works and along the way is able to offer flashes of revisionism. For example, he is able to question Davidsohn's and Volpe's idea of a broadly-based communal attack on ecclesiastical liberties in the early thirteenth century and even shows the commune helping the bishopric retain its jurisdiction in the Valdarno.

The evidential base for the author's thesis, however, is unfortunately a weak one. Although Osheim seems to have utilized with ingenuity every pertinent scrap of archival material, he himself still is forced to acknowledge repeatedly the fragmentary nature of his data—or indeed at times its nonexistence. For example, although there is evidence that the temporalities were rural based in the thirteenth century, there is no evidence that the temporalities were urban based earlier, and thus there is no support for the concept basic to his thesis of a ruralization of temporalities. The argument is based on an analogy with the experiences of other bishoprics, not on direct evidence concerning the Luccan bishopric. Moreover, lacunae in the state archives as well as in the episcopal archives seem to have shaped Osheim's conceptualization of the bishopric in a way that serves to give a sense of incompleteness to his picture. The process of transformation from civil power to rural lordship is surely to be understood in political as well as economic terms, as Osheim himself notes. Apparently because of a lack of materials, however, he focuses upon the economic aspects of change and touches only peripherally upon political issues such as military aid and taxation.

Given the limitations of the data base and the subsequent framework of analysis, Osheim's conclusions are perhaps best viewed as perceptive and valuable hypotheses comprising a model of development against which future studies of the Italian bishoprics must be set.

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JERZY MULARCZYK. *Dobór i rola świadków w dokumentach śląskich do końca XIII wieku* [The Selection and Role of Witnesses in Silesian Documents to the End of the Thirteenth Century]. (Prace wrocławskiego towarzystwa naukowego, Series A, number 189.) Wrocław: Ossolineum. 1977. Pp. 179. 32 Zł.

This book represents a fresh look at the process of choosing witnesses and at the role they played in verifying Silesian legal documents. The period of emphasis is the thirteenth century, though the collections of the twelfth century are introduced where they have survived. Jerzy Mularczyk bases the study on his University of Wrocław doctoral thesis. The book is an excellent example of thorough historical research, complete with extensive footnoting of sources, indexing, and a summary in German.

The first chapter deals with the documents held by the various reigning Piast princes to the year 1250. The documents were entrusted to the numerous Church institutions in the region, with the Cistercian collection proving the largest. Along with providing the structural network of princely documents, the work gives some indication of social and economic change in the region, particularly material growth, the increasing power of the Church, and the decline of feudal institutions, seen, for example, in the knights' desire for a change in their status to townsmen. The second chapter accounts for documents from the middle to the end of the thirteenth century and shows an increasing volume of witnesses' testimony. The author observed numerous inconsistencies that cast serious doubt upon the substantive worth of the many documents. Chapter three covers the Church documents of the Wrocław bishopric (1189–1300), and gives the reader an indication of the Church's hierarchical strata and function within the region. The next chapter covers the private lists of witnesses' documents. Though few in number, the documents again relate to the same personages appearing in the previous chapters.

The final chapter concerns the role of the witnesses. Here Mularczyk concludes that witnesses from the year 1230 onward were so closely connected with the upper levels of the Silesian Church's hierarchy that their testimony can only be suspect. Moreover, in several depositions the same monk's name appears as a witness.

Mularczyk's conclusions are undoubtedly controversial. His findings should stimulate renewed research by Western scholars of medieval Central Europe.

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TOMISLAV RAUKAR. *Zadar u XV stoljeću: Ekonomski razvoj i društveni odnosi* [Zadar in the Fifteenth Century: Economic Development and Social Relations]. (Monografije, number 6.) Summary in English. Zagreb: University of Zagreb, Institute of Croatian History. 1977. Pp. 325. ND 200.

The city of Zadar on the Dalmatian coast had a very turbulent history in the Middle Ages, having been dominated at various times by the Byzantines, Croats, Hungarians, and Venetians, while at the same time striving to preserve its municipal autonomy. Tomislav Raukar's book focuses its attention on the period after 1409, when Zadar definitely became part of the Venetian possessions. As its subtitle indicates, the book's main subject is a thorough study of all aspects of Zadar's economy and society in the fifteenth century.

The volume is divided into two parts and nine chapters. In the first part the author carefully surveys the economic, social, and political development of Zadar in the fifteenth century, including economic topography, urban growth, and demography. Furthermore, he studies the city's political history before 1409, its relationship with Venice, its administrative structure, social and ethnic relations, and migrations. The second part contains a detailed analysis of the economic activities of Zadar society, including agriculture, cattle breeding, salt production, crafts, trade, and navigation. The volume continues with a clear and well-written conclusion on Zadar society in the fifteenth century, a short appendix, a good, but too brief English summary, and a note on the author. It closes with an index that one wishes were not limited to just "the important names."

From the wealth of data some conclusions clearly emerge. Salt was the single most important article which influenced the fate of Zadar's trade. When Venice took over, in 1409, it curbed the salt trade and thus caused a general decline in the city's commercial activity. The other mainstay of Zadar's economy, agricultural land, remained important, but the Ottoman incursions, from the mid-fifteenth century on, had a negative impact here too. All of these developments affected Zadar society. The patricians, who up to 1409 were the richest and most active part of the population, lost a considerable portion of their power after that year, while the upper stratum of the citizenry emerged as the leading merchants and entrepreneurs. Raukar is fully aware of the role of Venetian policies in all of this, but he rightly points out that the old cliché that attributed all the ills to Venice alone can no longer hold.

Of course, even in a book this thorough one might wish to find more. Were there, for example, any slaves in Zadar? Why is there no separate treatment of credit and banking? It is also a pity that there is no map of the city in the book. Finally, the author's fine discussion of the general trends in the development of the economy and society of Zadar and Dalmatia might have profited from the modern works of some Western historians.

These are, however, minor points. Raukar's book is based on a vast amount of archival data, but they never obfuscate the main subject. His conclusions are cautious, sound, and well balanced. In short, this is an excellent book and the author deserves high praise.

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ERNST KITZINGER. *Byzantine Art in the Making: Main Lines of Stylistic Development in Mediterranean Art, 3rd–7th Century*. Cambridge, Mass.: Harvard University Press. 1977. Pp. xii, 175. \$25.00.

Ernst Kitzinger's considerable contribution to the scholarship of Early Christian and Byzantine art has, over the years, been highlighted by his continuing devotion to questions of form and by the unfailing subtlety and meticulousness that he brings to visual analysis. It is fortunate, therefore, that this eminent art historian was recently presented with an occasion, in the form of a series of lectures as Slade Professor of Fine Arts at Cambridge, to synthesize for a nonprofessional audience the fruits of more than four decades of stylistic criticism. The present volume is substantially based on those lectures.

As his subtitle indicates, Kitzinger is here concerned with the stylistic evolution of Mediterranean art between the third and seventh centuries, the critical period of transition from Antiquity to the Middle Ages. He devotes considerable attention in his introduction to laying out the methodological precepts behind the volume and to defining its goals. He believes that the formal aspect of a work of art holds important clues to understanding the age that produced it and that, over time, artistic form will follow a meaningful pattern of development in response to a changing *Zeitgeist*. His goal, therefore, is twofold: first, to discern within the profusion of seemingly unrelated and contrasting styles of the period an intrinsic pattern, and second, to elucidate that pattern in a broader sociohistorical context.

What Kitzinger actually finds is not a single grand "life cycle" of the sort generally thought to govern, for example, Baroque art, but rather a succession of contrasting dominant trends, which he conceives as interacting in a formal dialectic. In his view, the five centuries under examination are marked (as we see from his table of contents) by a series of actions and reactions, by "conflicts," "polarizations," and "syntheses." For example, official patronage around A.D. 300 consciously supports a primitive, expressionistic style in reaction to the cool, elegant style of the classical past, ap-

parently as a means of expressing to a wider public the superhuman strength and authority of the Tetrarchy. The following decades, on the other hand, are characterized by a stylistic reaction, by a gradually accelerating "regenerative" process through which classical esthetic values are reasserted. Among the leading forces behind this reaction is the pagan aristocracy of Rome, for whom, in the final decades of the century, classical styles and themes serve as weapons in their Kulturkampf against the onrush of Christianity.

The author continues through the fifth, sixth, and seventh centuries, identifying dominant trends and significant formal conflicts (e.g., "surface acceptance" versus "surface denial" in mosaic composition) and interpreting them, where possible, in historical, intellectual, and religious terms (e.g., as evocative of man's changing perception of and attitude toward the physical world and the forces governing it). In order to more subtly orchestrate this ongoing dialectic Kitzinger occasionally introduces the related concepts of "modality" and "inner- versus outer-directedness." The former is the idea that certain styles are inherently appropriate to certain subjects or media no matter what the age (e.g., a hieratic mode to the emperor; a classical mode to silver plate), while the concept of "directedness" concerns the differentiation between styles imposed by patrons and styles spontaneously developed within a closed artistic tradition. Both, assumedly, are responsive to the *Zeitgeist*.

Kitzinger's own style is both lucid and elegant. The book has been handsomely printed and effectively designed by the author's son, Stephen Anthony. The plates, which have been gathered together at the end of the volume, are so arranged as to visually reinforce the comparisons introduced in the text. It is concise, lightly but adequately documented, and eminently readable by specialist and nonspecialist alike. Methodologically, *Byzantine Art in the Making* is a milestone in art historical scholarship and is undoubtedly destined to become a standard work. The reader would be well advised, however, to bear in mind that Kitzinger is here attempting a synthetic overview of early Byzantine art whose scale is unprecedented. In order to find his formal dialectic the author has, by necessity (and by his own admission), been highly selective both in his choice of monuments and in the methodology that he has applied to them. Kitzinger is convinced that form has meaning and that style is propelled by some inner momentum through significant change. Certainly few periods show such a profusion of contradictory (and controversial) artistic impulses as that bounded by Constantine and Iconoclasm. Moreover, what little art has survived is often so poorly docu-

mented as to preclude conclusive dating and localization. Only someone with the breadth and depth of Ernst Kitzinger's erudition could even attempt such a synthesis.

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FRANZ TINNEFELD. *Die frühbyzantinische Gesellschaft: Struktur, Gegensätze, Spannungen*. (Kritische Information.) Munich: Wilhelm Fink Verlag, 1977. Pp. 394. DM 28.

Anyone can complain about the choice and balance of material in a brief handbook intended to reach a nonspecialist public. But an analysis of early Byzantine society without the emperor or the army, in which the sociology of the holy man or the monk gives way before a historical survey of the establishment of orthodoxy comprising nearly a third of the book, might seem to give the general audience rather an odd impression and leave some of the most central questions unanswered.

The method is indeed descriptive rather than analytical, and this perhaps is where the trouble arises. For despite much useful information, where exactly are the contrasts and tensions promised in the book's subtitle? Why, after all, did this society undergo such basic transformations in the seventh century, so soon after the date (A.D. 610) selected as the stopping point? Somehow in all the careful subdivisions the sense of a dynamic society has slipped away, the more so since Franz Tinnefeld does not provide a conclusion that would draw the threads together.

The book falls into four main sections, covering in turn land-holding, the senatorial class, the social structure of urban life, and what are called religious groupings from Constantine on. It was a bold gesture to attempt so much in such a small compass; unfortunately the contents list makes all too clear what has been omitted. Thus the economic basis of early Byzantine urbanism is never made clear; and while political aspects of the senatorial class loom large, we miss a discussion of how the bureaucracy worked, or of how bishops came to assume in the sixth century many of the functions of civic officials. The role of the people, promisingly allotted a fair amount of space, turns out to be discussed only in relation to the factions. In the chapter on towns, virtually no archeological material is used, and the author disappointingly limits himself largely to those major cities for which modern discussions are already available. We get very little sense of the Eastern empire in late antiquity as a conglomeration of rather small towns and villages always there in the background of the great urban centers. In the chapter on religion one

feels most the lack of focus on the social role of the institutional Church on the one hand and monks and holy men on the other, for example on culture and social mobility, and finds most surprising the lack of reference to works by Peter Brown and W. H. C. Frend.

These complaints, especially in so short a review, must not outweigh the merits of so compact a collection of material. But the reviewer is left suspecting that the general reader will not find here what he most wants to know, and wondering whether a period defined as lasting from A.D. 330 to 610 and treated effectively as a unity is really a plausible habitat for the creature we now tend increasingly to label "late antique man."

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ANGELIKI E. LAIOU-THOMADAKIS. *Peasant Society in the Late Byzantine Empire: A Social and Demographic Study*. Princeton: Princeton University Press, 1977. Pp. xiv, 332. \$20.00.

Although medieval historiography has focused increasingly on social and economic considerations and has done so with sophisticated methodologies, Byzantine history has remained remarkably conservative. To be sure, there have been some exceptions, and it must be noted that the surviving Byzantine sources are not such as to encourage the kind of treatment that is attempted elsewhere; there are, for example, relatively few documents which permit quantitative analysis, and almost none from before the twelfth century. Aside from a few items such as the Cadaster of Thebes, most Byzantine documents of this kind are preserved in the archives of monasteries, particularly those on Mount Athos. Such considerations, both in terms of the available sources and in the development of Byzantine historiography, are necessary for a full appreciation of Angeliki Laiou-Thomadakis' book on the Byzantine peasantry, since this represents both a major contribution to our understanding of Byzantine social history and an important breakthrough in methodology.

The book, however, is not a synthetic study of Byzantine rural life throughout the empire, but is restricted totally to Macedonia in the first half of the fourteenth century. Such a narrow focus is dictated by the evidence which forms the basis of the study, the monastic *praktika*, or inventories, which are particularly plentiful for this period. Within these relatively narrow chronological and geographical limits, the *praktika* provide remarkably full information about all aspects of social and economic life: households are listed according to

village, and within each household there is considerable detail about the property of the family and its individual members.

Appropriately enough, Laiou-Thomadakis has quantified much of this evidence. From the *praktika* of five monasteries she selected over fifteen hundred households in more than thirty separate villages. This material represents the main sample; evidence from other households was considered but not examined in the same way. Simple quantitative procedures allowed the author to measure some sixty-three variables within the sample; these included such information as the year of the *apographe* (census), village and household time-series forward and back, size of household, and number of individuals within various age categories. Most of the important conclusions of the book are drawn from this quantitative data, although the author has skillfully interwoven evidence from the literary sources in her analysis.

Early in the book the author reveals her primary theme and main conclusion: the Byzantine peasantry was becoming poorer and experiencing demographic decline as early as the first half of the fourteenth century. If this is correct, and the evidence seems persuasive, our evaluation of the difficulties of the Palaeologan period and the ultimate collapse of the empire will have to be modified in this light. The study argues that, contrary to what might be expected, the demographic decline did not improve the legal status or prosperity of the peasants who survived; instead, in nearly every way the peasant appears to have been worse off in 1350 than he was in 1300. The quantitative material, of course, cannot be expected to provide a clear explanation for these phenomena, but Laiou-Thomadakis suggests that causes are to be sought in the Catalan invasion at the beginning of the century, the civil wars which afflicted the middle of the century, and the short-sighted rapacity of the landowners, monastic and lay alike.

Beyond these major considerations, the study examines a whole host of subsidiary issues. Perhaps most interesting in this regard is the analysis of names contained in the *praktika*. Not only does the author provide a study of the movement of population (based in part on toponymics as proper names) and an analysis of the occupations practiced in the villages (based on occupational words used as names), but she also provides valuable insights into daily life and simple belief in a number of ways. Thus, she notes the baptismal names that were most common and those that were unusual, suggesting explanations wherever possible. Information of this kind will be of value not only to historians but also to cultural anthropologists.

It is, of course, easy to find fault with a book such as this. The style is difficult and there is

considerable repetition from section to section, making reading very tedious. Nevertheless, the most serious criticism is that a great deal of effort, and many pages of text, were expended to produce conclusions that were probably fairly obvious at the outset: "The life of the Macedonian peasant in the fourteenth century was very difficult." At first, this reviewer was inclined to object that the author did not go further with the interpretation of the data. In the end, I have concluded that such a conservative approach was probably the wisest course, since she has created a work that will serve as a useful resource for other scholars while providing a model for future research.

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MODERN EUROPE

JEAN-CLAUDE MARGOLIN, editor. *L'Avènement des temps modernes*. (Peuples et civilisations, number 8.) Paris: Presses Universitaires de France. 1977. Pp. 771.

Annalisme has demanded that the most famous French textbook series, "*Peuples et Civilisations*," bring itself up to date; and here we have the results of the demon of progress in history for the period that still claims to represent the beginning of "modern times." Some of what can be said in praise of the present volume was already suggested by Preserved Smith, who reviewed its predecessor for this journal (*AHR*, 35[1929-30]: 328-30) almost half a century ago. Smith congratulated the distinguished authors, Henri Hauser and Augustin Renaudet, for realizing that history was no longer monopolized by politics and religion and for attending also to economic, social, and intellectual matters. The present work builds upon that realization, pushes further those concerns, increases the division of labor (six scholars replacing the original two), and likewise flirts with what Smith referred to as "the dangers of the passion for using the very latest information." Remedies for two of the particular shortcomings noticed by Smith in the earlier work have been generously supplied. The neglect of "superstition" has been more than made up for in Jean-Claude Margolin's chapter on "*Sciences et croyances populaires*"; indeed the balance seems to have tipped in favor of sorcery, alchemy, magic, and astrology. For Smith "the most serious lack is that of demographic statistics of any sort"; this Jean-François Bergier tries to remedy in an introductory section which, using "the very latest information" and inquiring into environment and economic life, represents the clearest example of *annaliste* orientation.

The authority and accomplishments of the six men responsible for this text guarantee the high quality of scholarship and judgment for which this series (usually regarded, it seems to me, as the best of the various graduate manuals) is famous. Besides the general introduction, the chapters on economic history, including discussions of "*le premier capitalisme européen*," "*les trésors d'outre-mer*," and "*la première révolution industrielle*," have been undertaken by Bergier; the political history of the various European states, including the "margins of Europe," by Charles Verlinden; the history of art by Albert Chatelet; the religious movements of the Reformation by Jean Boisset and Bernard Vogler; and intellectual history, broadly construed, by Margolin, who is also the general editor. Speaking for this *équipe*, Bergier declares that the intention in general has been to give "*une dimension nouvelle*," especially the quantitative, in contrast to other manuals, which are "*trop exclusivement événementiels*." And indeed the rhetoric as well as the spirit of Fernand Braudel informs several sections of the book.

Yet the claims of novelty are overstated, for in many respects this work displays very conservative qualities. To begin with, the decision was made to retain the tripartite structure and many of the categories of Hauser and Renaudet. The result is an even greater lack of coherence than in the original and the appearance of an assemblage of monographs selected on the basis of current fashion. Bergier's prose is marred by the repetition of statistics contained in the very useful tables (at least in the introductory chapters). Verlinden is immune to this but only because he presents an utterly conventional political narrative somewhat at odds with editorial claims. His bibliography is also very sparse (especially in contrast to those of Bergier and Margolin). Conventional, too, and likewise bibliographically meager is the treatment of the Reformation by Boisset and Vogler, which is strong on lay piety and ecclesiastical abuses but rather perfunctory on the intellectual and institutional background (late scholasticism and conciliarism, for example). Margolin's chapters include very knowledgeable and suggestive discussions of European humanism (especially heavy on Erasmus, as might be expected), national literatures (where the quantitative instinct, in terms of bibliography and name-dropping, seems to be indulged a bit too much), and assorted topics in the history of knowledge and thought. It may be said here that some subjects seem to receive less than their due—for example, political thought, Machiavelli being discussed very sporadically and Claude de Seyssel barely mentioned (and then identified as "Guillaume"). But on the credit side we are treated to interesting and innovative essays

on such topics as utopianism, "folly," and childhood.

In general this book is a very self-confident, informative, often intelligent, and intermittently *au courant* set of discussions of aspects of history reflecting priorities recognized in French scholarship; but at the same time an impression is given of uncertain form and uneven substance. It is no doubt a virtue that the book is less committed than its predecessor to themes of expansion, enlightenment, and what Hauser called "*la modernité du seizième siècle*"; but the diffuseness not only of values but of any sense of relevance and coherence also gives the work a curious appearance of conceptual poverty. The controlling ideas in economic history still proceed from Marx and Weber ("*mal compris*," Bergier recognizes); and while we are assured that Calvin was *not* the "prophet of the industrial era" or a precursor of socialism, the bourgeoisie is restored to center stage as the savior of an "underdeveloped economy" ("*Marx ne s'y est pas trompé, qui a vu dans cette époque le premier accomplissement des ambitions bourgeoises*"). Burckhardt's "Renaissance" is also endorsed in general terms, but in view of embarrassing countercurrents, arising largely from popular and religious sources, the alternative idea of "anti-Renaissance" is suggested. To the vital question, "*Rupture ou continuité par rapport au passé?*" the innovative answer is given, "*rupture et/ou continuité*." Finally there is the possible objection of "*Européocentrisme*." Is this so? "*Certainement pas*," we are assured, "*mais . . . l'avènement des temps modernes, dans le cadre d'une histoire totale, est une histoire de l'Europe*." The conclusion is hard to quarrel with, but we are left with questions of our own. New information? Certainly. New dimensions? Sometimes. A new interpretation of European history? I don't think so. A new view of total history? Certainly not.

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JOSÉ L. DE ORELLA Y UNZUE. *Partidos políticos en el primer Renacimiento (1300-1450)*. (Publicaciones de la Fundación Universitaria Española. Monografías, number 24.) Madrid: Fundación Universitaria Española. 1976. Pp. 536.

José L. de Orella y Unzue has chosen a rather intriguing title for his monograph—*Political Parties in the First Renaissance, 1300-1450*. Although it may stimulate the curiosity of the reader, the caption does not prepare him or her for an introduction to the history of the Western church in the Renaissance (assuming, of course, that this is what the author means by his rather unfortunate phrase, "*historia eclesial del Renacimiento*" [p. 7]). Yet this is

what the work basically is—a textbook on the crisis of the late medieval Church, designed to complement Orella y Unzue's classroom lectures.

But this is only the first of the author's twin goals. The second is far more ambitious and explains the presence of "political parties" in the title. According to Orella y Unzue, the history of the Church between 1300 and 1450 can be understood in terms of the struggle of "ideological pressure groups" (p. 7). These groups ("social and ideological parties") behaved and functioned as true political parties within the Church, with their own distinct ideologies, techniques for political action, and programs. The result was what he calls the Church's "theological pluralism" (p. 8). With this party framework in mind, Orella y Unzue divides the history of the Church during this period into two "phases." In phase A (from Boniface VIII to the Great Schism) the party of the academic canonists predominates. Ideological preeminence in phase B, on the other hand, passes from the party of the cardinals (1378–1408) to the moderate conciliarists (1408–22) to the extreme conciliarists (1422–48). The end comes with the triumph of the papalist party in 1449.

The author's program compels him to meet two often conflicting sets of conditions. On the one hand, to fulfill his commitment to a textbook the author must offer the student a well-organized and systematic presentation of factual and conceptual developments. Such a requirement mandates having recourse to broad and necessarily oversimplifying generalizations. And this is precisely what the author cannot afford to do if he is to reach his second goal, for if on the other hand he is to convince the reader that the Church factions were indeed political parties he has to analyze a multitude of intricate factors, both spiritual and secular, operating from within and without the Church.

It is the first goal which Orella y Unzue meets best. The organization is excellent. The bibliographies, specialized and placed at the beginning of the chapters, are detailed and helpful. As expected, the author has paid close attention to the contribution of important Spanish figures who are often neglected in similar works. In short, Orella y Unzue's work should be counted as a significant contribution to Spanish textbook literature and ought to be welcomed by Spanish-speaking students.

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JOHN SEKORA. *Luxury: The Concept in Western Thought, Eden to Smollett*. Baltimore: Johns Hopkins University Press. 1977. Pp. xv, 340. \$17.50.

John Sekora has written a study of the writings of Tobias Smollett and in particular of his novel, *Humphrey Clinker*. He maintains that Smollett is a first-rate social and political satirist, and that *Humphrey Clinker* is literature of the highest quality. To a mere historian who is not a literary critic, this would appear to be the usual overestimation of a lesser figure who was presumably the subject of a doctoral essay.

Sekora tries to show that Smollett used the idea of luxury as the basic principle of his criticism of English society and as the organizing principle of his novel. So the first half of the book is a brief history of the idea of luxury from "Eden to Smollett," and the model is Lovejoy's *Great Chain of Being*. Sekora makes vast claims for the importance of the idea of luxury: "the single most important social and political idea of eighteenth-century England" (p. 9); "the charge of luxury was the most incisive criticism that could be directed against Western civilization" (p. 48); to account for the transformation of the idea in the eighteenth century, "one would need the grand cultural analysis provided by Kuhn in *The Structure of Scientific Revolutions* or Foucault in *The Order of Things*" (p. 112).

Unfortunately, luxury is not a coherent concept like the great chain of being. Hume is quoted as saying, "luxury is a word of uncertain signification" and Sekora proves the truth of that statement by making luxury synonymous with sin, original sin, greed, lust, ambition, pride, avarice, envy, disobedience, vice, drunkenness, corruption, and bribery. Any attempt to write the history of so amorphous a concept must end in a hopeless muddle.

Sekora does make some interesting points. In traditional societies, luxury was anything that tended to upset the established hierarchical order. The starving peasant who rebelled, the middle classes who grew rich and challenged the political monopoly of the old landed classes, statesmen and social theorists who believed in economic progress and hoped to increase happiness by making everyone richer were all charged with the sin of luxury.

It was right, said the traditionalists, that the poor should be poor; and that the middle classes should stay "middle"; and right only for "people of family and fashion to live up to their quality and fortune" (p. 90). This was not an unreasonable attitude, perhaps, for societies which had no way of increasing production, no way of preventing occasional famine, and which needed a powerful and conspicuous ruling class to maintain order. Any other attitude would be, as Sir Thomas More said, utopian.

But all this was upset by the economic revolution of the eighteenth century and by the Mande-

villes, Humes, and Adam Smiths who believed in the possibility of economic progress, who equated happiness with consumption, and who thought that every man should try to rise in the world. For these men "luxury" was synonymous with "refinement" and "civilization."

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B. F. PORSHNEV. *Tridtsatiletniaia voina i vstuplenie v nee Shvetsii i Moskovskogo gosudarstva* [The Thirty Years' War and the Entrance of Sweden and the Muscovite State]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 433. 2 r. 15 k.

Boris Porshnev's three decades of writings about the Thirty Years' War, now available in one book, are a most interesting contribution to the study of that conflict. Viewed from distant seventeenth-century Moscow, the war looked different from the way it appeared to more direct participants. Viewed from Stalin's Moscow (and Porshnev wrote out of that environment), it looks even stranger. We shall no longer, however, be allowed to ignore either the Muscovite or the Soviet viewpoint. Porshnev's assertions, presented with disarming wit and charm, are bold challenges to our conventional historiography. Based, reportedly, on sightings in archival materials, they will appeal to believers in conspiracy theories of history, class struggle, and Russia's importance, and, like the UFOs that plague another part of our establishment, will never be disposed of completely.

The great conspiracy, of course, was that of the German Habsburgs and their Spanish cousins, the pope, the Jesuits, the king of Poland, and other running-dogs of universal monarchy or Catholic Counter Reformation. Porshnev deftly weds the most extreme fulminations of Swedish and other Protestant polemicists of the seventeenth century to the Muscovite Polonophobia of the time, and more than a little World War II bitterness, in order to present a plot of monstrous dimensions.

The class struggle is a secondary theme for Porshnev but an important one. This aspect of the Thirty Years' War is presented as only a hopeless renewal of Engels' *Peasant War* of a century earlier. The supposedly extraordinary brutality of the war, and even many of the military operations, are explained as due to the desire of the threatened upper classes to crush and punish peasants. Porshnev is armed with an impressive catalogue of "struggles" but does not fully succeed in correlating class warfare with the overall course of events. That may be because it can't be done. A more direct reason, however, is that Porshnev's main

interest lies in establishing another correlation—the events in Germany (and, indeed, throughout Europe) are tied to a beat from Moscow.

The implementation of the Habsburg-cum-Catholic plot awaited, we are informed, the turns and twists of the Polish-Russian contest in the east. Thereafter, all the efforts of the anti-Habsburg forces, including those of Richelieu, were heavily influenced by the news from Moscow. And the core of the Porshnev argument is that only Russian subsidies and the Russian decision to attack Poland-Lithuania made possible the victorious Swedish intervention in Germany. The thesis is elaborated in great detail. Incredibly, not just the general development, but every move Gustavus Adolphus made, every advance, every pause, is explained in terms of the progress of negotiations with Moscow, in terms of the king's related fears and ambitions vis-à-vis Poland, in terms of grain shipments to Amsterdam, etc.

Portions of the Porshnev thesis were confronted and rejected by Western historians long ago—not least at Stockholm in 1960. His general propositions will probably be rejected on general principles. But the detail will have to be dealt with in detail—and even if much of it is never accepted as valid history, all scrupulous historians of the Thirty Years' War hereafter will have to ask themselves at one point after another whether Moscow had a role in it or not.

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MARTIN BERGER. *Engels, Armies, and Revolution: The Revolutionary Tactics of Classical Marxism*. Hamden, Conn.: Archon Books. 1977. Pp. 239. \$15.00

Although minor compared with Marx's achievement, Engels' contribution to their cause was considerable. The limits as well as the extent of that contribution are expressed in his assuming the military responsibility in the making of revolution. Engels had entered into a status deeply and even humiliatingly subordinate to that of his masterful friend; arms, which bored Marx while arousing the *homo ludens* in Engels, were to be obedient to the civilian idea. Yet arms *were* irreducibly important. Acceptance of this subordination was the best feature of Engels' military service to the revolution and suggests the scenario for the roles, in circumstances favorable to action, of Lenin and Trotsky.

Martin Berger properly commends Engels (p. 63) for integrating his military thought "into the vision of revolution"—but Berger continues—"that he and Marx developed." Nowhere has Berger demonstrated that Engels shares credit for the Marxian world-historical vision. Nothing that

Engels wrote has the greatness of view which Marx was demonstrating at least as early as 1844, in the *Economic-Philosophical Manuscripts*, nor can any significant parts of Marx's vision be traced to collaboration with Engels. Berger has yielded to the natural temptation to magnify his subject's importance.

In fact, as Berger accurately records, Engels, an extraordinarily facile writer, followed his initial and fundamental correctness on military matters with a profusion of errors. Enjoyably playing soldier (Marx's daughters nicknamed him "the General"), he set himself up as a military commentator. From his comfortable Manchester and London residences he wrote dispatches on the Crimean and Franco-Prussian wars, among others, which were outstanding for their wrongheadedness. In January 1871, a week before the fall of Paris, he still thought France had a good chance to avoid defeat. More ambitiously, Engels developed ideas on military organization, conceiving of a proletarianized army that would conquer the capitalist state from within. Berger does not quite know what to do with thinking so lacking in seriousness.

Berger, failing to apply sufficient rigor to Engels' military adventures, has written a diffuse and inconclusive account. A clearer analysis would not have denigrated Engels. Even his many errors on the subject had their value, since they made the Marxian cause more aware of the function of the military in the world. The executive officer of the revolution won his medals honestly enough.

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SALO W. BARON and GEORGE S. WISE, editors. *Violence and Defense in the Jewish Experience: Papers Prepared for a Seminar on Violence and Defense in Jewish History and Contemporary Life, Tel Aviv University, August 18-September 4, 1974*. Philadelphia: Jewish Publication Society of America. 1978. Pp. xii, 362. \$12.00.

From the very beginnings of their history, Jews have been both the subjects and objects of violence and have had to formulate appropriate ethical, religious, and defensive responses. It is ironic that this nomadic, desert nation, which was forged in the violent conquests of Canaan, experienced the role of victim of violence and oppression in much of its remaining history. In the nineteen-hundred-year Diaspora of the Jewish people which culminated in the destruction of the Holocaust, Jews as a widely scattered and vulnerable minority group have had to deal with violence as a fact of daily life. In the process, they developed certain approaches

and strategies of defense designed to facilitate survival. Until now, there has been no significant literature on the basic Jewish ideology and approach to violence. This vacuum in scholarship is partially filled by this volume, which emerged from a comprehensive symposium on the subject held at Tel Aviv University in 1974. The resulting book represents an unprecedented effort to assess this important issue in Jewish history in a systematic fashion. It is noteworthy because it focuses attention on this problem and because it relies on a multidisciplinary approach using the insights of Biblical scholarship, Jewish philosophy, sociology, political science, and history. Precisely because of the wealth of scholarly talent represented in this volume and the importance of the subject, however, some of its failings are magnified.

The book is divided into two parts: the ancient and medieval periods, and the modern and contemporary periods, each introduced insightfully by Salo W. Baron. In part one, Harry Orlinsky places Jewish violence in the context of the biblical covenant relationship. His argument that Jewish aggression in this period must be seen in the framework of the biblical imperative is problematical, since all nations have a nationalistic ethos which justifies violence under certain circumstances. Yohanan Aharoni presents archeological evidence which suggests that Canaan was not vanquished by violent conquest and internecine warfare, but rather by gradual infiltration. Ephraim Urbach analyzes practices and attitudes in the Hellenistic and Talmudic periods. Joel Kraemer and Shimon Shamir examine the precarious situation of the Jews in Islamic countries, but essentially add little to the literature on Islamic anti-Semitism produced by S. D. Goiten, Hayim Cohen, and Albert Memmi; and Emanuel Rackman, in perhaps the best essay in the volume, provides an overview of the problems in reconciling violence and the Jewish celebration of life from a Halakic perspective. Rackman's essay, unlike some of the others, is comprehensive, conceptual, and analytical. His major theme, forcefully argued, is that at no time in Jewish history was *Kiddush ha-Shem* (martyrdom) thought of as preferable to self-preservation. There remains, however, a significant deficiency in historical comprehensiveness in this section as there is little discussion of the medieval Christian period.

Part two examines the ways in which violence has shaped Jewish life in the last two centuries. Uriel Tal discusses the centrality of anti-Jewish violence in Nazi ideology. The Jewish side of the issue, specifically the debate over Jewish responses during the Holocaust informed by people like Bruno Bettelheim, Alexander Donat, Terence Des Pres, and Victor Frankl, is not confronted. Mi-

chael Confino analyzes Soviet anti-Zionism. Ben Halpern and David Schers take up Jewish responses to anti-Semitism in the United States and Latin America. Graenum Berger considers the role of the communal worker in Jewish self-defense; and Haim Cohn examines Jewish law concerning violence and its relevance for Israeli society.

Although many of these articles are of a high quality, they mostly survey the problem and are not analytical and comprehensive. There is little continuity between the essays, and hence there is no general thesis that can be isolated except for perhaps the multiplicity of Jewish responses to violence. There are serious gaps in the treatment, notably in the areas of medieval Christian anti-Semitism, anti-Semitism generally, and in Jewish reactions during the Holocaust. Yet, the book is informative, suggestive of future research, path-breaking in its attempt, and a useful addition to the sparse literature on the topic.

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AUGUST BERNHARD HASLER. *Pius IX. (1846-1878), Päpstliche Unfehlbarkeit und 1. Vatikanisches Konzil: Dogmatisierung und Durchsetzung einer Ideologie.* (Päpste und Papsttum, number 12, in two parts.) Stuttgart: Anton Hiersemann. 1977. Pp. xii, 627. DM 300.

Pius IX enormously enhanced the power of the papacy within the Roman church. The major step in this development was the promulgation of the doctrine of papal infallibility by the Vatican Council in July 1870. No action, however, was so important in also making Pius' pontificate a byword for reaction and obscurantism. Yet for all the controversy which accompanied and followed such an extravagant theological formulation, the circumstances surrounding the council's decision remain obscure.

A major deficiency in all the writings about the council was that they rested on a slim basis of source material. The archival evidence is widely scattered, and much of it remains difficult—if not impossible—of access. But in the most recent attempt to reexamine the whole matter, August Bernhard Hasler, a Swiss priest and doctoral candidate in history at the University of Munich, has succeeded in utilizing to a greater extent than ever before a wide range of archival material located in England, France, Germany, Italy, and, insofar as permission was granted, in the Holy See itself. On the basis of this comprehensive research, Hasler provides a valuable and minutely detailed discussion of the council's internal deliberations and procedural irregularities. He painstakingly exam-

ines all the theological arguments for and against the dogma of papal infallibility, and he describes the manner in which the newly defined dogma was implemented by the Curia.

The results of Hasler's findings refute the "official" scholarship of the Roman church, especially the writings of Theodor Granderath and Cuthbert Butler. Hasler's book, as a result, confirms the viewpoints of the council's early critics who charged that improper pressure was brought to bear on the participants to influence them into defining the dogma of papal infallibility. Throughout the council, Hasler argues, a small but tightly knit group of bishops, actively supported by the pontiff, sought the proclamation of the dogma and manipulated the proceedings to neutralize all opposition. New evidence presented by Hasler also shows that Pius himself was the moving force behind the dogma and suggests that the pope was not of sound mind.

Hasler also dismisses as pious legend the orthodox view that the bishops opposed to papal infallibility—the so-called Minority—took such a position because they believed it was unwise and inopportune to make such a definition at that time rather than because the dogma was deemed theologically unsound. Hasler maintains instead that the Minority bishops were, with few exceptions, never intellectually convinced of the dogma's theological validity and in the end, when they gave their assent, their action represented no more than an act of submission to papal authority.

If Hasler's evidence is new and his conclusions revisionist, his methods nonetheless remain conventional and even old-fashioned. Despite the controversial attempt at psychohistory, the bulk of his analysis and discussion follows a familiar methodological approach. This methodology, unfortunately, gives scant attention to the political and social context in which the dogma was formulated. Religion and politics were inextricably intertwined in Pius IX's pontificate, and the pope's aims cannot be fully understood without reference to the larger events taking place in the Italy of the Risorgimento. Pius IX's experience with the revolution of 1848, the excesses of the short-lived Roman Republic, and the loss of the Papal States all influenced his policy. In confining his historical inquiry to the deeds of the small group of ecclesiastical leaders who monopolized decision making within the Church of Rome, however, Hasler largely excludes from consideration the underlying origins of the dogma. The actions of high churchmen are important in explaining what happened at the council, but equally significant are the social and the political factors which conditioned their behavior. Hasler's methods tell us more about how the council was maneuvered into defining the dogma

of papal infallibility than in explaining why such a definition was sought at all.

Nevertheless, Hasler has written an important and provocative book which takes its place along side the recent works of Brian Tierney and Hans Küng. That his conclusions have been the subject of attention in such popular news publications as *Time* magazine in this country and *Der Spiegel* in Germany testify to the interest in, and to the significance of, the issues Hasler has examined.

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JUKKA NEVAKIVI. *The Appeal That Was Never Made: The Allies, Scandinavia and the Finnish Winter War, 1939–1940*. Montreal: McGill-Queen's University Press. 1976. Pp. x, 225. \$14.00.

Earlier publications on the Russo-Finnish war of 1939–40 include the Finnish Foreign Ministry's *Blue-White Book* (1940), *The Memoirs of Marshal Mannerheim* (1953), Foreign Minister Väinö Tanner's *The Winter War* (1957), and Max Jakobson's classic study of *The Diplomacy of the Winter War* (1961). Jukka Nevakivi's craftsmanlike study of Anglo-French diplomacy during the conflict fills out the picture admirably.

The Soviet attack on Finland offered the Allied Powers an opportunity for diverting German pressure from France to Scandinavia, which was the French objective, and for cutting off Germany's vital iron ore imports from Sweden by occupying the ore fields, which was the British objective. They hoped to achieve both by obtaining troop transit rights from Norway and Sweden under the cover of a League of Nations call for assistance to Finland, but their requests were rejected. They then tried to persuade the Finns to make the appeal, but the Finns refused, both because the help offered was inadequate and because they guessed what the real Allied purpose was. In March the Finns made peace with the Russians, to the consternation of the Allied Powers.

This is the story that Nevakivi uncovers in all its fascinating detail. Based on intensive archival research in Helsinki, Stockholm, Oslo, London, and Paris, interviews with participants, and rich published materials, the study is as definitive as possible. Of particular interest (and novelty) are the chapters dealing with the Allied intervention plans, the difficult decisions which had to be made in Helsinki, and the collapse of Allied hopes. Also of great interest is the epilogue on Allied public opinion and unofficial assistance during the war, although it comes as a rather artificial conclusion to the book. One can only hope that Nevakivi's

sequel to this study, *Ystävästä viholliseksi: Suomi Englannin politiikassa 1940–1941* (1976), also will appear in English translation soon.

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GEOFFREY COX. *The Race for Trieste*. London: William Kimber. 1977. Pp. 284. £5.95.

In April 1945 the Western Allies raced the Yugoslavs to see who would reach Trieste first. Geoffrey Cox described this contest for the first time in his book, *The Road to Trieste* (1947). The book has ever since been a valuable source on the meeting in Trieste of the Allies and Tito's forces. Cox was well qualified to depict these events for he had been senior intelligence officer of the Second New Zealand Division, part of the British Eighth Army, which had entered Trieste as the first Western force on May 2.

In his second book, *The Race for Trieste*, Cox uses, in addition to his diary, top secret British documents, specifically those of Prime Minister Winston Churchill, made available only recently. Furthermore, to the description of the advance of the British-American forces in the west he has added that of the Yugoslav Fourth Army east of the Adriatic, the latter based on the official Yugoslav military history. This new material provides a much broader background for the contest; it shows that the decision as to who would occupy and hold Trieste was determined not solely on the battlefield but primarily by the statesmen of the big powers—Churchill, Truman, and Stalin. For these reasons *The Race for Trieste* should be regarded as a new book rather than a second edition of the first.

From the newly released documents we learn interesting new facts or confirm old assumptions. Thus we find that the Soviet disregard of the Yalta agreements on Poland occasioned Churchill's decision to back Italy against Tito's advances in Venezia Giulia and Trieste, particularly, after the Western statesmen learned of the arrest by the Soviets on March 28, 1945 of the non-Communist Polish underground leaders whom the Russians had induced to go to Moscow under written guarantee of safety. Cox adds that the arrest of these Polish leaders—before World War II ended in Europe—can be regarded as the beginning of the Cold War. The author also describes another interesting occurrence. When the Western Allies demanded the retreat of Tito's armed forces from Trieste during the first days of May 1945, Churchill had previously received reports from Field Marshal Alexander, commander of British troops in Italy, and from Ralph Stevenson, the British am-

bassador in Yugoslavia, that Tito was not sure of Soviet backing should a clash with British-American troops arise. Cox explains that neither Alexander nor Stevenson indicated the origin of his information, and speculates further that it must have derived from either intelligence reports or from "instinctive judgement" based on many months of dealing with Tito. The documents also disclose that the phrase "iron curtain" and its concept were used for the first time in Churchill's telegram to President Truman on May 12, 1945, and not a year later in Churchill's speech at Fulton, Missouri, as is commonly accepted.

Cox is to be congratulated for this excellent new book.

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ROBERT K. MERTON and JERRY GASTON, editors. *The Sociology of Science in Europe*. (Perspectives in Sociology.) Carbondale: Southern Illinois University Press. 1977. Pp. xiv, 383. \$19.85.

In the preface, the editors note two important historical moments in the emergence of a sociology of science: "Just as the sociology of science has come to avoid the parochialism of impermeable disciplinary boundaries, so it had some time before avoided the parochialism of national boundaries." In one sense, an interdisciplinary approach appears to be second nature to most sociology of science, in the most general usage of the concept. Yet, in "France, as elsewhere," Paul Frank ("The Sociology of Science in France"), observed, "the sociology of science is not a well-defined subject." Barriers created by national and linguistic realities remain a source of concern to practicing sociologists of science. While research published in English enjoys a wide audience, work published in languages other than French or German receive less attention. This problem, discussed at many world congresses of the International Sociological Association from 1962 on, suggested the "idea of having a volume that would inform the growing numbers of scholars . . . about work being variously developed in European countries."

Divided into two parts, the second section contains two-thirds of the volume, and includes seven essays in English covering the historical development and present state of the field in eight nation-groupings (Britain, Italy, West Germany and Austria, Poland, France, USSR, and Scandinavia). Included as an appendix is a bibliography of sources on Hungarian research. Collectively, the authors pay varying degrees of tribute to the many founding fathers—Saint-Simon, Condorcet, Comte, Marx, and Weber, and to their likeminded

offspring, Sarton, Merton, and Kuhn, among others. One illustration of national bias is provided by Rolf Klima and Ludgen Viehoff ("The Sociology of Science in West Germany and Austria"), who demonstrate that Marx and Weber are the ideological and methodological patriarchs for practitioners in those nations.

The reader is impressed with the uneven interest expressed over time and in different nations in the possibility of a viable sociological subspecialty with a focus on such questions as: Who are scientists? What do they think? What do they actually do? How and where do they conduct their activity? As early as the 1920s, for example, the work of Znaniecki and the Ossowskas provided most of the ingredients—theoretical framework, procedures, and subjects—for study in Poland, and perhaps elsewhere, but conditions favorable to growth soon disappeared from Europe in the following decade. During the 1960s, scholarly productivity in most nations, including the United States, increased almost in proportion to the growth in international contacts produced by the world congresses. By describing the intent of his essay on Britain as "informative rather than critical," M. J. Mulkey captures the major reason for publishing this collection. In addition, the editors' comments implicitly suggest the future appearance of another volume that would contain studies in the sociology of science in the Netherlands, Czechoslovakia, East Germany, Belgium, Romania, and Yugoslavia.

Similar to other state-of-the-art pieces is Merton's episodic, autobiographical memoir, which constitutes part one of the book. This 138-page "oral and documentary history" of the field (primarily the United States), and Merton's role in it, is quite lively and valuable especially to scholars in other fields who wish to learn more about it. Merton, a prime mover himself, describes with pride the ideas and work of a group of physically isolated yet dedicated professors as they evolved into an "institutionalized and cognitively identifiable field of inquiry" by the 1960s. He notes with sorrow that some opportunities for interdisciplinary scholarship were lost to that old nemesis, disciplinary provincialism. During 1965–66, for example, Charles C. Gillispie, then newly appointed editor-in-chief of the *Dictionary of Scientific Biography*, tried to convince his advisory staff of fellow historians of science that the volumes of the *DSB* should be prepared with the more thorough requirements of a computerized version in mind; archives of biographies, printed and stored also on magnetic tape, would benefit both the sociologist and the historian of science. The plan was stillborn, yet it could have produced a "computer archive of biographical information ready for (prosopographical) analysis of a scale and depth never before achievable."

Merton hopes that new interest, undaunted by rigid disciplinary boundaries, will revitalize the plan.

The authors in this volume would all agree that the sociology of science has achieved more than academic adolescence. Yet, all would quickly add that much empirical research and theoretical construction is needed for healthy growth. The extensive bibliographies of pertinent sources after each essay serve as indicators of the field's vitality throughout Europe and in the United States.

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ROBERT C. JOHNSON *et al.*, editors. *Commons Debates, 1628*. New Haven, Conn.: Yale University Press. 1977. Pp. xiii, 136; 584; 641. \$12.50; \$32.50; \$35.00.

The history of Parliament in the early Stuart period has been of special interest to American scholars for more than half a century, and Americans have taken the lead in editing and publishing the source materials. As early as 1921, while he was still at Minnesota, the late Wallace Notestein brought out a critical edition of the Commons debates for 1629, and in the next decade, during his tenure at Yale, he supervised preparation of the splendid seven-volume set of debates for 1621. Two volumes of proceedings for 1610 were edited by Elizabeth Read Foster and published by Yale in 1966. At about that time the Yale Center for Parliamentary History was founded with J. H. Hexter as its presiding genius. The present volumes are the first fruits of a continuing project which should eventually give us texts of the journals and diaries for the remaining sessions held between the accession of James I and the coming of the Civil War.

The materials for 1628 will fill five volumes. The first three have been published simultaneously; the last two are promised shortly. Of these volumes, the first is a valuable introduction describing the individual diaries, the Commons Journal (which is newly edited and reprinted), and the so-called "Proceedings and Debates." The fifth volume will be a comprehensive index. Volumes 2, 3, and 4 provide the edited materials themselves.

The editors follow Foster rather than Notestein in arranging their matter day by day rather than diary by diary. As they say, this system makes it far easier to see what was happening at any given time; it tends to obscure the peculiar flavor of individual accounts, but on balance it is probably the more convenient way. Spelling has been modernized, and every help has been provided for non-specialists. The standard of editing and production is very high. The volumes are lavishly printed, with ample margins and the delightfully old-fash-

ioned device of thumb notches to assist in speedy location of materials. The editors have compiled a helpful "Order of Business" for each day of the session, and they provide a carefully argued discussion of the very important "Proceedings and Debates," which they suggest was a semi-official compilation of proceedings in committees as well as in the House itself, probably put together by the clerk, John Wright, and his son. Occasionally the introduction seems pedantic and unnecessarily detailed (do we really need to know the color of the leather binding on the original manuscripts?), and the editors do not seem to be on as sure ground in references to Tudor parliamentary history as they might be (their definition of *praemunire* is singularly inadequate, and they do not seem to know that the borough of Gatton was in the hands of the Copeleys well before Elizabeth's accession). The House of Lords is given short shrift, and it seems unfair to consign the Lords' materials to an appendix of the *Common Debates*. But these are petty points; there is remarkably little ground for quibbling.

It is impossible in a short review to suggest all the fascinations which lie in this newly accessible material. The principal achievement of the 1628 session was the famous Petition of Right, and one can now see it taking shape, gradually, in debate and in amazingly frequent conferences with the Lords and even with the King. One is struck by the quality of debate and by the legal learning displayed in oratory. When else did a legislature have a contingent of scholars like Coke, Selden, Littleton, and Hakewell? On the other hand, one cannot but note the small quantity of routine legislation produced: Parliament had changed between 1529 and 1628 from a body dealing expeditiously with an enormous bulk of bills into a grand national debating society. Finally, one must be impressed by the vividness of several of the private accounts. John Newdegate's diary in particular captures much of the spontaneous interchange in the House and thus much of the real character of the session.

It is pleasant to note that the publication of these materials was aided by the National Endowment for the Humanities but painful to read Hexter's account of his troubles in securing adequate funding. Editorial work of this quality is costly, but it should never have to be done over. It merits the support and appreciation of all historians.

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Twin Cities

MURRAY TOLMIE. *The Triumph of the Saints: The Separate Churches of London, 1616-1649*. New York: Cambridge University Press. 1977. Pp. xii, 251. \$18.95.

Earlier historians such as W. T. Whitley, R. W. Dale, and Champlin Burrage wrote primarily from a denominational perspective in exploring the antecedents of Congregationalists and Baptists, but in 1936 Christopher Hill called for greater attention to ideas and environment, and less to "tracing the genealogy of sects." Recent scholars have been particularly attracted to the ideologies of Levellers, Diggers, Fifth Monarchists, and Ranters, leaving Murray Tolmie troubled by the lack of attention to "respectable nonconformity" (an odd charge considering the contributions of B. R. White and G. F. Nuttall). In this book Tolmie writes essentially in the Whitley-Dale tradition, presenting a scholarly account of the development of nonparochial Protestant congregations in London, commencing with Henry Jacob in the early 1600s and concluding with the split between the Particular Baptists and Levellers in 1649.

In 1616 Jacob organized a secret congregation in London, independent of the parish churches but unwilling to repudiate them. His church split in 1630 when John Duppa led a secession of strict separatists opposed to the latitudinarian semiseparatism of John Lathrop, Jacob's successor, who himself took thirty members to New England. The remnant appointed Henry Jessey pastor in 1637, but for reasons of secrecy and convenience this church divided in 1640, with Praise-God Barbone assuming the pastorate of one group, the prototype of subsequent gathered churches under lay leadership. Before 1638 John Spilsbury, perhaps seceding from Duppa's church, organized a congregation practicing believer's baptism, which drew additional members from Jessey's church. In 1641 Richard Blunt led a major secession from Jessey's congregation to found another Baptist church, which divided when some members doubted the authority for their baptism and became Seekers. Despite such losses, the strength of the Particular Baptists increased until they had seven London churches by 1644. Tolmie retells the story of the origins of the General Baptists in the contacts between John Smyth and Thomas Helwys with the Waterlander Mennonites in Amsterdam.

In the later chapters Tolmie argues that the Independents' millenarianism facilitated unity with militant Presbyterians to form a clerical war party, using apocalyptic language to create emotional unity. Yet as the Independents increasingly favored a nonparochial gathered church instead of parochial congregationalism, they split with the Presbyterians. Tolmie dubiously asserts that during the course of this evolution the semiseparatist statements of the Independents were a duplicitous plot to deceive the Presbyterians and preserve the anti-episcopalian alliance and the parliamentary war party.

By 1646 London had thirteen Independent gathered churches, nine separatist churches with lay pastors, seven Particular Baptist churches, and five General Baptist churches. Until late 1645 the Presbyterians hoped to accommodate the Independents in a parochial framework, but failure led to a conservative attack, typified by Thomas Edwards' *Gangraena*. Consequently Independents, separatists, and Baptists loosely organized to work for toleration. In part this entailed alliance with the Levellers, but as the political Independents and the New Model Army assured religious toleration, the Independents and sectaries refused to support the Leveller program. The saints, Tolmie contends, formed their own political party which was at odds with the Leveller ideal of a secular state, and this party in turn allegedly provided support for army leaders in the 1650s.

Tolmie's work rests on an assiduous examination of printed source materials, though familiarity with modern scholarship is inadequate. Due attention is not devoted to the ideological and experiential foundation of Protestant dissent, the importance of which is manifested in the studies of Nuttall, White, Leo Solt, Sears McGee, T. L. Underwood, and others. There is more to the history of separate churches than Tolmie's arid opening chapters reveal. These objections aside, this is a meritorious book which deserves a sequel dealing with the 1650s, but not restricted to London.

RICHARD L. GREAVES
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J. G. A. POCOCK, editor. *The Political Works of James Harrington*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press, 1977. Pp. xviii, 878. \$49.50.

This is the first edition of Harrington for over two centuries, and a very good one it is. The text is established with meticulous care. Proper respect is shown to Harrington's first editor, the underrated John Toland (pp. xiii, 141-43). In the 150-page introduction the circumstances of publication are carefully established, with useful information about John Streater, Livewell Chapman, and the possible ex-Digger Richard Goodgroom (pp. 9-12). *Oceana* is set in what the editor believes to be its ideological context, and an interesting conclusion surveys the fate of Harrington's ideas after his death.

The introduction contains many good things. J. G. A. Pocock is interesting on Marchamont Nedham, "a chief precursor of Harrington's *Oceana*," though he fails to note that Nedham's phrase "kingly power" was a favorite of Gerrard Winstanley's (pp. 34-35). Justice is done to Henry Neville,

"whose originality as a thinker should not be lightly dismissed" (pp. 134-36). Pocock makes sense of the way in which Harrington came to argue that Scotland, Ireland, and other nations might be "liberated" by falling under English hegemony (pp. 72, 113). He makes remarks *en passant* which show that he has a much better understanding of the theology of Winstanley, for instance, than many who have written on the subject at length (pp. 73, 80, 96). He notes how Harrington's prose style became clearer and more readable in 1659 (p. 101) and attributes this to Neville's influence. But a similar simplification of Milton's prose in the same critical year is usually explained by an urgent desire to get his ideas across to the widest possible audience—a desire which Harrington certainly shared.

One is so grateful to Pocock for this edition that it seems churlish to carp. Yet, since all students from now onwards will come to Harrington through Pocock, it must be said that the latter's approach is idiosyncratic as well as ingenious. It is that of a historian of ideas concerned to give Harrington a pedigree. We can all agree on Harrington's debt to Machiavelli, and Pocock produces convincing evidence for suggesting a closer relationship to Nedham than had been assumed. But there is something very arbitrary about the idea of an "ideological context." Pocock, for instance, suggests that Harrington was attacking the theory of the balanced constitution put forward in the *Answer to the Nineteen Propositions*, written in the King's name by Falkland and Colepeper in 1642 (pp. 19-22). For this there is no evidence whatsoever. It may even be true. But the document is stressed, I suspect, partly because it has become fashionable among historians of late, partly because its constitutional emphasis fits in with Pocock's interpretation of Harrington. Unless some evidence is produced to establish that A is influenced by or replying to B, there is a danger of playing intellectual games. Pocock's regrettable decision to omit one of Harrington's writings, the very short *Mechanics of Nature*, obscures Harrington's place in another chain of ideas, extending from the Hermeticists to eighteenth-century deism (currently under investigation by M. C. Jacob).

Pocock seems to me to underestimate the influence of the world in which Harrington lived. Tawney long ago noted anticipations of Harrington in Raleigh, Bacon, Sir Thomas Wilson, and Goodman; we might add Sir John Davies' *Microcosmos* of 1603, Spelman, Quarles, Henry Parker, the anonymous *Certificate from Northamptonshire* (1641), and Tom May's *History of the Parliament* (1647). Pocock depicts Harrington as an armchair political thinker, who "very likely never heard of the debates at Putney"; but he nevertheless quite rightly

suggests that "*Oceana* is an attempt to provide a theory" of the kind that Ireton propounded against Rainborough in the Putney Debates (pp. 27, 42).

But other significant events took place in the forties and fifties. One was large-scale confiscation and sale of lands in England and especially Ireland. Another was the abolition of feudal tenures—surely crucial to Harrington's thinking, but something which Pocock does not mention. He refers indeed to proposals for emancipating Scottish tenants from control by landlords and chiefs, but "no study of the pre-history of the Army's perception of Scottish affairs is likely to reveal that Harrington was deeply immersed in the climate of regimental radicalism" (p. 43). Perhaps not; but it is at least interesting that in September 1646 and October 1648 "the plebs" in the Army was putting forward schemes for an agrarian law, soon to be so important for Harrington (see J. A. F. Bekkers, editor, *Correspondence of John Morris with Johannes de Laet, 1634-1649* [1970], pp. 122, 149).

Pocock seems to think that Harrington was so oblivious to what was happening in the economic sphere that the word "capitalism" is irrelevant to his thought (pp. 56-63). Yet Harrington's emphasis on the transfer of land to "the people" must surely be related to the fact that capitalism in England was mainly rural. Pocock admits that a passage on page 840 is difficult to reconcile with his thesis; he might also have considered pages 293, 304-05, 405-07, 430, and 470. Use of the secret ballot, so essential to Harrington's system, derives from the practice of merchant companies and the City of London.

Pocock seems to me too easily facetious about Harrington's prediction that monarchy and the House of Lords could never be restored in England. Kings after 1660 were not monarchs in Harrington's sense; they were princes in a commonwealth (pp. 351-52, 608). The restored peerage was very different (in Harringtonian terms) from that which had existed before the abolition of feudal tenures, before financial control passed from monarch to Parliament (cf. pp. 198, 703, 770).

So one could go on arguing. Pocock's provocative introduction has made the discussion necessary; his admirable edition has made it easier to conduct. Perhaps he would make the next edition even better by reconsidering two of his notes. The reference on page 447 to Frenchmen driving out "the Spaniard and the English" would best relate to the fifteen-nineties, when both nations were invading France simultaneously. Harrington's phrase on page 738, "a house [of Parliament] elected by the counties only" would fit the Barebones Parliament; it might glance at the Parliaments of 1654 and 1656, elected under a constitu-

tion which greatly reduced the number of borough representatives.

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KATHERINE S. VAN EERDE. *John Ogilby and the Taste of His Times*. Folkestone, England: William Dawson, distributed by University Press of Virginia, Charlottesville. 1976. Pp. 183. \$20.00.

John Ogilby (1600–76) emerges from the pages of Katherine S. Van Eerde's biography as a man of resilience and business acumen. Ogilby's career was filled with pitfalls: as a dancer in the entourage of the Duke of Buckingham he suffered an injury leading to permanent lameness; his career as a theatrical producer in Dublin was terminated by the fall of his patron the Earl of Strafford; his manuscripts, including an epic poem on Charles I, perished in the Great Fire of London. Yet this poor Scot of obscure birth died in prosperity as a successful translator, publisher, and printer.

Unfortunately for his subsequent reputation Ogilby suffered the fate of most of those who were consigned to the realm of dullness by Dryden and Pope. The eighteenth-century regarded him with casual contempt; later generations forgot him. Van Eerde demonstrates that Ogilby, who taught himself Latin and Greek in middle age, produced respectable translations of Virgil, Homer, and Aesop. Her contention, however, that Ogilby's poetry fell victim to a change of "taste" in the Augustan Age is less convincing. Poets do go in and out of fashion, but seventeenth-century poetry which was the result of true creative work continues to be read.

It cannot be said that Ogilby defined taste for his own age, but he was astute in fulfilling the desires of the reading public. Thus he produced his translations of the classics for royalist patrons sidelined during the 1650s and cashed in on the Restoration with his account of Charles II's coronation procession. In the latter part of his career he catered to increased interest in the world beyond Europe with his handsome atlases. They were outstanding examples of bookmaking, the coffee-table books of the period, but offered little in the way of original scholarship. The exception was his *Britannia*, with its accurate maps of English and Welsh highways and incomplete "surveys" of the natural history and antiquities of English counties, which owed much of its seriousness of purpose to Robert Hooke.

As befitting the life of a great bookmaker, this is a particularly handsome book graced with many illustrations from Ogilby's publications. Van

Eerde has made an energetic search for materials on Ogilby, but his personality remains shadowy. Ogilby's historical significance is that of an entrepreneur of letters achieving prosperity by shrewdly playing the game of patronage and responding to the tastes of the reading public.

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PETER EARLE. *Monmouth's Rebels: The Road to Sedgemoor, 1685*. New York: St. Martin's Press. 1977. Pp. xi, 236. \$13.95.

In the introduction to *Monmouth's Rebels*, Peter Earle states, "My main aim in writing this new book about Monmouth's rebellion was to try to discover a little more about the sort of people who took part in this last purely English rebellion in English history" (p. xi). The book lives up to that charge; Earle's research into Quarter Sessions Records, the "Monmouth Roll," and local archives allows him to produce a composite portrait of the men who risked their lives and property to support Monmouth in his attempt to overthrow James II. The author insists repeatedly that both the rebels and the rebellion have been misrepresented and that too many historians have accepted the idea fostered by the Stuarts that the rebels were ignorant peasants or malcontents who were seduced into a hopeless rebellion by the young, romantic Duke. *Monmouth's Rebels* makes sure a new picture will emerge: many of the rebels were men of property and standing; they were mature men, some of whom had fought in the civil wars, and they were convinced they were fighting for the protection of the true Protestant faith in England.

The valuable lesson to be learned from the very important appendix, in which he presents the statistics to support his analysis, is how difficult it is to deal with population figures in the seventeenth century. He states that his sample is sufficient for a "crude analysis" (p. 200) of the situation, but the conclusions are sophisticated and far reaching and require great confidence in the applicability of the figures.

One of Earle's more striking claims is that the West Country was the natural place for the rebellion to occur because that area had suffered more during and after the civil wars than had East Anglia, the other major center of Nonconformity. This suffering hardened the resolve of the men of the West Country, and they seized the chance to rebel when presented with it. Surely, it is reasonable to assume that East Anglia would be receptive to rebellion precisely because it had not suffered the horrors of war, had not been punished severely

after 1660, and the heirs of the Cromwellian tradition (a group Earle thinks was important in the rebellion) had seen the hand of God at work in the victories of the 1640s.

Earle deals well with Monmouth's complex personality, pointing out his strengths as well as obvious shortcomings. Monmouth recognized the existence of the resentment against James and took advantage of the situation. Soon after the rebellion started, however, he lost confidence in the ability of his army to win the kind of victory he had foreseen and his irresolution and lack of nerve doomed any chance he had of success.

After we see the new picture of the men who fought at Sedgemoor, we must face the inescapable fact that it was "Monmouth's rebellion," not just another uprising of the enthusiastic, the radical, or the nostalgic. Earle concentrates too much on destroying his own interpretation of a "Whig view" that the rebellion failed because it was not led by the right sort of gentlemen. His own conclusion that the rebellion had a chance to succeed, but not much chance of making a success of government, raises a crucial consideration by hinting at the internal contradictions of the rebellion. It is unfortunate that this observation appears in the last paragraph of the text and is used as a final salvo at the presumed orthodoxy, rather than as the basis for a new analysis.

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PATRICK CROWHURST. *The Defence of British Trade, 1689-1815*. Folkestone, England: William Dawson and Sons. 1977. Pp. 281. £8.00.

Recent interpretations have emphasized the institutional requirements of European economic growth. Overseas trade, for example, could grow in the seventeenth and eighteenth centuries only as pirates ceased to be the unwelcome beneficiaries of the traffic. Only more effective enforcement of property rights induced shippers to increase the scale of their ventures on the high seas. And the costs of enforcement often determined the institutional arrangements: for years the English found it less expensive to pay the bribes demanded by the Barbary pirates than to provide naval escort for merchant traffic in the Mediterranean.

Patrick Crowhurst describes the institutional arrangements that brought British merchants more effective protection against pirates and privateers in the eighteenth century. In the opening and most useful chapter of the book he argues that privateers from St. Malo, Bayonne, and Dunkirk posed the most serious threat during this period of inter-

mittent Anglo-French war. To deal with the menace the Admiralty began to organize convoys in the 1690s, and, according to Crowhurst, improved the effectiveness of this service during the eighteenth century. A chapter on insurance sketches the early history of Lloyd's and suggests that merchants, insurers, and the Admiralty cooperated in enforcing convoy discipline.

The chapters that deal with specific trade routes fail to develop a convincing argument. Crowhurst offers a general description of the New England economy rather than an assessment of how well naval forces protected trade, perhaps because, as he admits in mid-course, no reliable statistics for shipping losses exist. He attributes the success of the tobacco and sugar trades in part to the provision of convoy escort but once again cannot offer the evidence needed to isolate a causal relationship. The final chapter begins with the familiar history of the East India trade and then drifts off into a tedious description of the routes taken by the merchantmen.

Crowhurst offers no conclusions about the efficiency of the various defensive measures. He assumes that convoys were essential to the prosperity of the merchants but does not estimate the costs which resulted from this technique as sailings were often delayed and markets were regularly glutted. The provision of naval escort might have been in fact an inefficient allocation of resources: large, well-armed East Indiamen could usually defend themselves even in European waters, and it is worth asking if the use of such ships, rather than convoys of smaller vessels, in the tobacco and sugar trades would have been more efficient, charging the consumer instead of the taxpayer with the costs of protection.

To the constant annoyance of the reader, the book is not edited. A map of the Baltic includes none of the place names mentioned in the text. Table 6.2 is mislabeled. "Real value" is confused with "market value." "Commerce" is repeatedly used as an adjective. One must continually supply punctuation and untangle syntax in an attempt to get at the author's intent, and only the most intrepid reader will be able to keep his sea legs to the end of Crowhurst's voyage.

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CHARLES K. HYDE. *Technological Change in the British Iron Industry, 1700-1870*. Princeton: Princeton University Press. 1977. Pp. xvi, 283. \$18.50.

In an industrializing economy the capital goods industries play a strategic role, for the general capacity to increase the rate of economic growth is

closely linked with their evolution and progress. Even productivity gains made in agriculture and its associated processing industries are, to a great degree, determined by improvements in tools and machinery which, in earlier times, were often made of iron. Charles K. Hyde's study has been eagerly awaited by those who are familiar with his earlier publications in the periodical literature, and there can be no doubt that this book is an important, original, and substantial contribution to our understanding of the development of an important industry during the British economy's drive to industrial maturity. Many earlier scholars—Ashton and Birch being two who were brave enough to produce general surveys—have examined particular aspects of the story in exercises often devoured by controversy, but Hyde has produced a new synopsis and, in the course of preparing it, has used much primary material previously unexploited. Moreover, the scenario is essentially quantitative with a solid supporting cast of economic theory.

All aspects of the iron industry's development in the period under review have been carefully surveyed. The first part of the book is primarily concerned with the change from a charcoal-based industry to the era of coke smelting, and the factors behind the changing fortunes of the various iron-producing regions are analyzed with admirable intellectual rigor. The questions asked of the data are important, precisely framed, and scrupulously answered. Capital costs, production costs, prices, and markets receive careful attention, as do various explanations relating to the diffusion of technology. However, the significance of the regional ironmasters' associations in maintaining the economic existence of marginal producers might have been examined more closely.

The second part deals with the mature iron industry in the period 1815 to 1870, assessing particularly the development of the smelting sector and the regional changes consequent upon the adoption of the hot-blast process. The latter greatly reduced fuel costs but also favorably affected capital costs and labor productivity, since daily furnace output could be substantially increased. However, the continuing expansion of the pig-iron industry is explained without much reference to labor supply or cost. This is perhaps a minor quibble, for the general analysis of the implications of increased output at lower cost is exceedingly good. Moreover, the expansion of the wrought-iron sector is viewed in the proper context of a rapid growth of pig-iron production, a natural but commonly ignored symbiosis. For most British pig iron went to the forge, often in regions different from those that produced it.

Central to the book is the study of the develop-

ment and diffusion of new production techniques. Hyde advances three major conclusions: 1.) technical advances, both in the pig-iron and wrought-iron sectors, were relatively slowly diffused; 2.) initial change was followed by continuous refinements; and 3.) the application of new production methods was determined by their profitability. His research perhaps makes entrepreneurial behavior appear too logical, but this reviewer would not fault that trend in scholarship. The mercantile side of the story, never adequately treated, was not really Hyde's province; one suspects, however, that he believes (pp. 197–98) that his conclusions might be modified, if only slightly, by further research into the operations of the iron market. Distribution, the supply of capital to the producers, and the provision of entrepreneurial skills were often closely linked in merchant partnerships. Changes in these, often accompanied by the poaching of managers and skilled labor from established iron-working districts, must also have affected the pace of technical change and the diffusion of innovation, particularly in the earlier period. This book will stimulate further discussion and research, and its methodology is a model to those who will follow.

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LUCY M. BROWN and IAN R. CHRISTIE, editors. *Bibliography of British History, 1789–1851*. Oxford: Clarendon Press. 1977. Pp. xxxi, 759. \$44.00.

H. J. HANHAM, editor. *Bibliography of British History, 1851–1914*. Oxford: Clarendon Press. 1976. Pp. xxvii, 1,606. \$73.50.

The publication of these two bibliographies on British history virtually completes an Anglo-American project that started at the beginning of this century. The entire span of British history from its earliest times to the eve of World War I has been captured in a full series of Oxford University Press volumes. Edgar Graves' volume (1975) covers the period to 1485, updating Charles Gross' first work of the series; Conyers Read's volume (1st ed. 1933, 2d ed. 1959) covers the Tudor period; the Stuart period was compiled by Godfrey Davies in 1928 and revised in a second edition by Mary F. Keeler in 1970; the eighteenth century to 1789 by Stanley Pargellis and D. J. Medley appeared in 1951. The two works under review now join their companion volumes to form one of the most authoritative and valuable bibliographical sources in the field of British history.

Both the Brown/Christie and the Hanham volumes maintain the excellence of the series. They follow the general format, style, and typography of

the others. Brief descriptive, and sometimes evaluative, remarks follow many of the entries. The table of contents of each is lengthy, detailed, clear, and well organized and, in effect, represents a subject breakdown of the field. Each has a comprehensive and detailed index for both subject and author. A judicious use of both the index and the table of contents will permit the researcher to exploit these works to their fullest extent.

The previous volumes in the series have run from about 4,500 to 7,200 items or entries in each. Thus the Lucy M. Brown and Ian R. Christie volume, with 4,732 entries is similar in scale to the others. But commentary appended to many of the numbered items often contains further references to books, pamphlets, articles, journals, etc., so that the actual number of works cited far exceeds that figure. The H. J. Hanham volume covering 1851-1914 reflects the explosive increase in modern historical writing. With 10,829 numbered items (to which add the thousands of additional references in the commentaries) and 1,606 pages, including an index of 367 pages, it is a massive and monumental accomplishment.

Anyone, of course, will be able to find omissions in these volumes. A favorite book, a significant article, or an indispensable citation will no doubt be missing. Some will think that certain areas or topics have been given insufficient attention—in my own view, for example, more might have been done on local history. But a bibliography cannot possibly encompass in detail the entire contents of a reference library. In the end, the test of a good bibliography is not its comprehensiveness but surely its usefulness as a guide for the student, the scholar, and the researcher to further sources and materials on the subject of study. These two works amply meet that test. They cite all the major bibliographical aids and they list the principal standard works. They cover not only the well-known fields of politics, diplomacy, and economics, but also such less-trodden paths as Victorian mountaineering, animal welfare, and engraving and lithography. These bibliographies fulfill their prime function as ideal starting points for almost any research project in modern British history. The two volumes also reflect new interests in historical research. Works on women's history now appear in separate sections on women and children, on women's suffrage, on working women, and on the status of women. Urban history and the history of science now get more space.

The ideal bibliography should be like a living plant, ever growing, multiplying, branching. Occasionally it should be thoughtfully pruned. Unfortunately and inevitably it is in the nature of a printed bibliography to suffer from a case of arrested development, no longer able to grow,

though capable of being replanted into new editions or of receiving grafts of supplements and addenda. Hanham's, for example, includes items published up to 1972, some for 1973, but none thereafter. The problems of bibliography-making are being compounded by the enormously increased output of published material. Indeed, it is unlikely that any printed bibliographical undertaking of the scale of this series can be practically or economically carried out in the future. The next great bibliography of British history will no doubt be the product of the computer. In the meantime, these two works, alongside their companion volumes in the Oxford series, will long remain as valuable tools for the student of British history.

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J. S. COCKBURN, editor. *Crime in England, 1550-1800*. Princeton, N.J.: Princeton University Press. 1977. Pp. xiv, 364. \$18.00.

This is a collection of eleven essays variously related to crime and criminal law administration in early modern England, the purpose of which, according to the editor, is "to promote a dialogue between scholars" and "to provide an authoritative and easily accessible base for further study" (p. xiii). Those are not necessarily complementary undertakings, and the first is given priority, as J. S. Cockburn appreciates, for he confesses to slight depression over the book's topical and spatial limitations (only six counties are treated in any detail) and warns that the essays may seem excessively local and conservative. He defends the enterprise by stating that the essays are almost entirely free of ideological biases and then makes the scientific claim: "Here manuscript sources have been allowed to speak for themselves . . ." (p. xiv).

G. R. Elton, in a critical introduction, finds "mildly anachronistic confusion" (p. 3) in the collection, a result of treating crime as a social phenomenon rather than approaching the subject through the administrative machinery. He is far from finding the essays free of all ideological predilections, with the manuscripts performing a capella.

The editor's attempt to provide overarching purposes and Elton's predominantly negative assessment (shaped by his own predilections, and surely better suited to a concluding commentary than to an introduction) are discouraging beginnings to a fine collection of essays, all but one of them solidly grounded on archival research. J. H. Baker provides a useful introduction to criminal law administration, the only essay to span the period covered by the book. Cockburn follows with a cautious

survey of the nature and incidence of crime in Essex, Sussex, and Hertfordshire (1559–1625). A. D. J. Macfarlane has a lucid essay on witchcraft in Tudor and Stuart Essex, a topic he has helped to make familiar elsewhere. J. A. Sharpe focuses on crime in just one Essex parish (Kelvedon Easterford) in the early Stuart period; M. J. Ingram examines the interrelationship of litigation to communities in early seventeenth-century Wiltshire; and T. C. Curtis has chosen Cheshire to explore how and why individuals came to appear before Quarter Sessions in the seventeenth century. The preceding three essays are local and conservative certainly—indeed, Sharpe self-deprecatingly describes his work as “an anecdotal account of the doings of obscure persons” (p. 107)—but the overall result is informative.

The eighteenth century is treated in the remaining five essays. Choices of topic and a generally less conservative approach make these essays the most stimulating. J. M. Beattie’s essay on crime and the courts in early eighteenth-century Surrey, complementary to his earlier work, is an excellent analysis of how the perceived character of the defendant and changing social conditions—from the expansion of London to the effects of peace and war—influenced the administration of justice. R. W. Malcolmson treats infanticide and reveals the hazardous circumstances of many females in domestic service; P. B. Munsche studies the ineffectual attempts of country gentlemen to enforce the game laws in late eighteenth-century Wiltshire; W. J. Sheehan examines conditions in Newgate Prison, where money was the ultimate source of solace; and P. Linebaugh ends the collection with a well-written and perceptive examination of the Ordinary of Newgate, particularly the *Accounts* of the trials, lives, and executions of malefactors produced by the holders of that office. L. A. Knafla contributes to the book’s cohesion by providing an excellent critical bibliography. The book is well annotated and indexed. Rich in its various perspectives and approaches, this is a better book than Elton’s introduction suggests.

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A. P. DONAJGRODZKI, editor. *Social Control in Nineteenth Century Britain*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 258. \$16.50.

As Asa Briggs has shown, much of the social commentary of the early nineteenth century was marked by fears that social and economic transformations were undermining the connections between the rich and the poor. For centuries both had clung to an ideal that celebrated the recipro-

cities of the master-servant relationship. Within such a milieu those who remained beyond its sanction were numbered among the “disorderly,” and even in the eighteenth century there were many, from the denizens of outcast London to the dwellers of forest and fen. But faced with the massive encroachments of industrial and urban society in the nineteenth century, authority could no longer avoid the need to devise a new approach to preserving social cohesion. Increasingly, as face-to-face interaction gave way to more impersonal and less frequent contacts between rich and poor, social control was mediated through an elaboration and proliferation of formal, bureaucratized agencies.

The bulk of this collection of nine essays focuses on this development in considering the question of social control in nineteenth-century England (Scotland and Wales do not figure in the book, despite its title). The efforts of police and magistrates, clergymen, educators, state bureaucrats and reformers, poor relief functionaries, and social workers to impose or inculcate approved behavior in a changing society are dealt with in the contributions of John Stevenson, Richard Johnson, Jennifer Hart, Hugh Cunningham, Michael Rose, Judith Fido, and A. P. Donajgrodzki. Two of these essays deserve individual attention. Richard Johnson here continues his important work on the ideological content and motivation of educational policy in the 1830s. In portraying education as central to the creation of “conformities between the cultures of a social formation and the necessities of production” (p. 78), he offers a breadth of insight that is often lacking in histories of public education. In an equally stimulating piece, Judith Fido reveals the extent to which the development of social casework in the late nineteenth century was contingent upon, and linked to, the particular social theories of the Charity Organisation Society. Fido persuasively argues that casework, springing from a determination to “remoralize” the poor through personal contact and influence, provides an apt example of the uneasy relationship between social work and social control.

Notwithstanding these achievements, the chief flaw in the book as a whole stems from a preoccupation with “policing” in its wider nineteenth-century meaning. It may be that the advent of mass society compels attention to its institutional structure, but the reader becomes somewhat uneasy when the sort of social control explored here seems to leave so little room for the role of the controlled. For the most part the poor, the exploited, and the disaffected are inconspicuous in this book, which tends to minimize the degree to which social control can be a process of genuine interaction between classes and their cultural ex-

pressions. The major exception to this criticism, Victor Bailey's first-rate essay on the Salvation Army riots, shows how authority might be moved to strike a delicate balance between the expectations and views of the people and the need to maintain order and the interests of the elite. It is true that Bailey's work centers on social dynamics in the relatively unruffled waters of southern provincial towns, but even where change was more intrusive, popular culture might continue to shape authority's response. This is made clear in Robert Storch's fine piece on the "moral" reaction to working-class leisure. Although he claims to be concerned only with "the collective mentality of the early Victorian middle classes" (p. 138), his analysis is firmly rooted in the wider context of transformations in the nature and patterns of popular leisure and culture.

Something of a pioneering venture, this book is successful in dramatizing the need to consider more fully the sources of social stability in nineteenth-century England. As the editor, A. P. Donajrodzki, points out in an informative introduction, much remains to be done in the area of social control. He identifies three aspects of the problem that require further examination: the control dimensions of such social institutions as the working-class family, the different rates of social change in particular localities and their impact on control mechanisms, and the nature and limitations of consensus between classes. Only when such questions are elucidated can we begin to understand how the transition from a paternal to a mass society proved possible.

PETER DUNKLEY
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DAVID PHILIPS. *Crime and Authority in Victorian England: The Black Country, 1835-1860*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 321. \$17.50.

A growing rate of crime and politically inspired industrial violence, Victorians feared, threatened the existence of their social order. While Friedrich Engels saw pervasive crime as evidence of working-class contempt for bourgeois laws, Sir Archibald Alison, the Tory sheriff of Lanarkshire, worried about the incorrigible "dangerous classes" congregated in the populated manufacturing districts. David Philips assesses the validity of such perceptions to ascertain whether they were warranted.

Philips' method is to focus on the Black Country from the years 1835 to 1860 and to study that coal-mining and iron-producing area with its notorious (even for that time) hard-drinking, illiterate working class as a microcosmic urban-industrial society

that theoretically should have been rife with crime. He researched almost all available court and police records, and in the process he read, analyzed, and quantified twenty thousand cases committed to trial in that region.

What his vast research efforts unearthed about the nature of crime in the Black Country tends to minimize contemporary accounts of a criminal working-class population. Philips discovered there were relatively few murders and aggravated assaults. So free from violent death did Black Country people live, that the authorities executed (all executions then were public) only four persons between 1835 and 1860. During this period, it should be noted, both capital punishment and public hangings were under attack by a small but vocal and influential group of reformers; executions generally were reduced drastically.

Any statistics about crime are questionable and we know today about how police agencies inflate their crime rates both for public support and funding. Philips' awareness of this problem is reflected in a very thorough and revealing study of the kinds of crimes for which people were indicted. Crime, Philips points out, depended upon what the authorities decided was criminal, and what the authorities decided to prosecute were the traditional rights once possessed by the workers to such things as "black gleaning" coal tips and taking materials from work shops. Nascent capitalists, less interested in old relationships and more interested in profits, clashed with the habits of preindustrial communal perquisites. The masters decided to discipline their work forces by prosecuting workers for petty larceny that made up to eighty percent of the indictable prosecutions. Moreover, acts of Parliament made prosecutions cheaper and quicker by allowing petty-larceny cases to be heard in Petty Session before two justices of the peace. The court was soon dominated by the coal and iron masters who tended to become the justices of the peace and who obviously acted in their own self-interest.

Philips found that a professional criminal class was almost nonexistent in the Black Country, perhaps responsible for no more than ten percent of the crime. London with its commercial wealth, rookeries, and flash houses was the world of a professional criminal class about which Dickens and Mayhew wrote; but that was not the situation in the Black Country. Philips speculates that was probably not the situation in other industrial areas either. London was not England, but is it any more valid to equate the Black Country with other industrial areas? At any rate, Philips has made a case for the Black Country, and rather than discover a region on the brink of social disorder, we find crime which consisted mainly of paltry thefts,

and a working class which, for the most part, accepted the legal system.

The political and industrial violence of the 1830s and 1840s frightened the authorities, and so it should have. Insurrection and revolutionary activities in England and on the Continent were still fresh in the minds of men, and it is only through hindsight that we know that Chartism was more rhetorical than revolutionary. When in the 1850s fear of social disorder disappeared, the authorities shifted their concerns to crime as an issue separate from social and political unrest. "Crime," Philips observes, "was no longer seen as linked to political subversion, rebellion, social breakdown, but as a social problem which, like disease, bad housing or the poor would always 'be with us' " (p. 289), and crime became "normalized" at some point between 1830-60 the author concludes.

Philips makes a very forceful argument that the state of crime in early Victorian England was very much a state of mind. It would be ridiculous to assert that crime did not increase, and that is not what the author means; he is saying it did not reach dangerous proportions. There are unanswered questions, however, about the contention that the condition of crime in the Black Country was a general reflection of crime in England. Whether this was the case will depend upon the findings of other such regional studies. Meanwhile the author has made a contribution to an understanding of crime in one region. Philips has corrected many misconceptions, particularly about the system of private prosecutions as practiced then, and he has provided us with much useful information about the Victorian criminal justice system. It is regrettable that much of the tone of the book is didactic, but that may be the manner of revised dissertations.

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DORIS LANGLEY MOORE. *Ada, Countess of Lovelace: Byron's Legitimate Daughter*. New York: Harper and Row. 1977. Pp. 397. \$25.00.

In reading this fine account of the tragic life of Ada, Byron's legitimate daughter, I was reminded of Virginia Woolf's fantasy in *A Room of One's Own* about Shakespeare's having had an extraordinarily gifted sister called Judith. This creature, Woolf pretends, longed to practice her talent for writing, defied her parents' decision to betroth her to a neighboring wool-stapler, fled to London, and begged for training in acting, only to be impregnated by an actor-manager, and finally, in despair, took her own life one hostile winter's night. "For it needs little skill in psychology to be sure that a

highly gifted girl who had tried to use her gift for poetry would have been so thwarted and hindered by other people, so tortured and pulled asunder by her own contrary instincts, that she must have lost her health and sanity to a certain degree."

Ada was Byron's daughter, not his sister, and her passion was to be a creative scientist, not a writer; but Ada and the Judith of Woolf's fantasy were women with bonds. The brooding presence in Ada's life was her mother, the high-minded Lady Byron, a woman obsessed by the injury her husband had inflicted on her and determined to stifle in her offspring any hint of the traits of the infamous poet. Ada, born in 1815, never knew her father, as he separated from her mother when she was just thirty-six days old. But the child early displayed an intensity and eccentricity which labeled her her father's daughter, and something of her own—a love of mathematics. In her teens, she studied the subject and wrote amusingly to a friend in the style of an epistolary novel: "So this you see is the commencement of 'A Sentimental Mathematical Correspondence between two Young Ladies of Rank' to be hereinafter published no doubt for the edification of womankind. . . . Ever yours mathematically." Ada had grand and unrealizable plans to develop what she believed was a unique scientific talent ("I hope before I die to throw light on *some* of the dark things of the world"); and she eagerly sought instructors, including Charles Babbage, the famous mathematician and father of the modern computer, who became her close friend.

Ada made a good marriage at age nineteen to Lord King, the future Earl of Lovelace, to whom she bore three children in four years. Then, tormented by the interruption of her studies, she sought tutoring and fed her fading ambitions with increasing quantities of opium and alcohol. Her mother, meanwhile, gained ascendancy over her household and grimly assumed the reclamation of one happier than she, another family member called Elizabeth Medora Leigh, whom Byron had assumed to be his daughter by his half-sister and who had gone on to an involvement with her brother-in-law, which produced three more illegitimate children. While Lady Byron made meddling into a divine calling, Ada carved out a pathetic space for her own thwarted talent: with her skills in numbers she worked out an elaborate betting system and managed secretly to place borrowed money on horses. Heavy losses then opened her to blackmail by her go-betweens, at least one of whom was also a lover. Broken in spirit, and always frail of body, Ada died in 1852, aged thirty-six, of uterine cancer.

The book is based on previously unpublished family papers, and although the sheer amount of

detail is occasionally overwhelming, it provides a most instructive spectrum of women's lives in an aristocratic and blighted family.

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D. C. M. PLATT, editor. *Business Imperialism, 1840-1930: An Inquiry Based on British Experience in Latin America*. New York: Oxford University Press. 1977. Pp. xv, 449. \$28.50.

Twenty-five years ago, in a celebrated article entitled, "The Imperialism of Free Trade," Gallagher and Robinson advanced the thesis that the mid-Victorian period was the high point of British expansion. British commerce and finance penetrated into all parts of the world, backed when necessary by political influence and sometimes by naval and military force. Since then there has been an outpouring of books and articles supporting or disputing this thesis. Among the leading critics has been D. C. M. Platt, Professor of Latin American History at Oxford. Platt has maintained that the evidence does not support the contention that there was a deliberate governmental policy during this period to seek favored treatment for British traders. He has backed his position with impressive documentation. The present volume, edited with an introduction by Platt, contains a collection of case studies by various scholars on trade and finance in Latin America. Their conclusions generally support Platt.

It is impossible within the brief compass of this review to provide an analysis of the eleven essays which comprise this book. They deal with finance, public utilities, shipping and railways, coffee, beef, nitrates, and other subjects. They all involve detailed investigation of extant company and governmental records, the kind of factual material which is so often lacking in the debate on the nature of nineteenth-century imperialism.

In general, the rates of return on investments in trade, railways, and public utilities were somewhat higher than the yield from British and colonial government securities, but the differential was only about one or two percent. There were some notable exceptions—the Liverpool Nitrate Company paid dividends of one hundred percent or more six times between 1905 and 1913—but this return was in marked contrast to the general level of six to seven percent.

Rory Miller describes the relationship between British firms and the Peruvian government in the years after Peru lost its guano and nitrate to Chile. He examines the thesis accepted by a number of writers that British commercial interests abroad

depended heavily on the political collaboration of a local elite, and finds it greatly overstated in a Peruvian context. In the years he studied there was never a complete identity of interest between the Peruvian government and British firms. He also makes the point that "government" involved not only the executive branch but a Congress, which was often fractious even when the executive was cooperative with foreign firms.

In another particularly interesting study, Robert Greenhill deals with the alleged British control over Brazil's external trade. He points out that only two British firms were in the top seven coffee shippers and that American and German companies were powerful competitors. Finally, the government moved in to establish a valorization policy that exploited Brazil's dominance among the coffee growers of the world.

The impressive array of scholarly research contained in the eleven chapters does not lead to the conclusion that British economic interests necessarily had a benign effect on Latin American societies. On the contrary, as Platt points out in his introduction, British enterprise "consistently outstayed its welcome, and came ultimately to serve as a barrier to, rather than the promoter of, Latin America's economic development" (p. 13). British commerce and finance reinforced the forces defending the status quo and thereby contributed to disequilibrium in Latin America.

These excellent case studies of business imperialism in Latin America provide a model which it would be profitable for scholars to follow in dealing with other areas of the world.

JOHN S. GALBRAITH
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PETER MELLINI. *Sir Eldon Gorst: The Overshadowed Proconsul*. (Hoover Colonial Studies.) Stanford: Hoover Institution Press. 1977. Pp. xiii, 315. \$10.95.

Students of British imperial history see Egypt from the 1880s to the 1920s as a special case, even more so than other short-term instances such as Iraq and Tanganyika. There had been national regimes in Egypt before the British came, occasionally unified and strong enough to undertake imperial adventures of their own. Sophisticated, partially-Europeanized societies existed, underlying a polyglot congeries of leadership groups with educational institutions, newspapers, and professional associations of a sort that took generations to evolve in India. If all this had not yet produced a solvent and viable nation state, capable of resisting threats from outside, it nonetheless constituted a far more outspoken and unruly community than the British

had to deal with in the subcontinent during a comparable period, not to mention in Malaya or Nigeria. In short, the components of workable imperialism were not there. The Egyptians were too weak to keep Europe at bay, but too self-conscious to be led quietly along the road of modernization. The imperialists, for their part, were not ensconced long enough to acquire the ruling attitudes of an ICS—self-confidence born of solid experience, and so close a knowledge of the people that extremes were instinctively avoided.

This book grew out of a Stanford dissertation. Peter Mellini, who teaches at Sonoma, worked in England and Egypt, using impressive primary sources: the private papers of his subject and of many other principals including Grey, Cromer, Milner, and Storrs, as well as parliamentary papers of the period. His aim is to present Gorst's career in the context of evolving policy with regard to Egypt and the empire in general. On Gorst's stewardship (1907–11) he is thorough and often instructive. Contrasting Sir Eldon's attempt at liberalization with Cromer's approach—English heads and Egyptian hands—he threads his way through the minefield of intrigue, emotionalism, naiveté, and misconception with perspicacity and skill. One may wish in fact that he had confined himself to Gorst's Egyptian career, with emphasis on the last four years, stressing Egypt's social makeup more than he has space for here, treating the British superstructure in greater detail and coming down hard on the minutiae of day-to-day interaction.

When talking of late Victorian society and of imperialism farther afield, the author seems less sure. The House of Commons, for example, had looked rather searchingly into imperial rule long before 1906. In places there is a tendency to start with clichés, to line up the material in conformity with them, and to apply the standards of a later day. Durrell's colorful brush is questionable here, especially when used in the text itself. Connolly's Eton, with all its charm, is less appropriate than any number of other examples that could be named.

The conclusions of this valuable book are just, for all their ferocity. Perhaps they, and the commentary throughout, would have been helped by a word on what Cromer and Gorst could have done differently, and with better effect, in the circumstances.

ROBERT HEUSSLER
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ROBERT RHODES JAMES. *The British Revolution, 1880–1939*. New York: Alfred A. Knopf. 1977. Pp. xiv, 653. \$17.95.

Robert Rhodes James sets out to pursue a "subtle political and social revolution" which was "vast" but "almost silent" (pp. xii, 618). So subtle was it, indeed, that little more is heard of it and the reader can be left to enjoy undisturbed an attractive and intelligent narrative of political happenings and the men behind them between the heyday of Gladstonian Liberalism and the coming of the Second World War. Not that the book is without social content: at certain points along the way Rhodes James stops and announces the importance of the economy, or intellectual currents or the suffragettes. But for the most part it is a political story that he wants to tell. The book becomes, therefore, a history of political dialogue—mostly at or near the top—and a study of the role of personality in history, a legitimate preoccupation for one who cut his teeth on biography.

Already the strengths and weaknesses of *The British Revolution* are implied. The character sketches and appreciations of politicians have the lightness of touch and the power to convince that one would expect from an accomplished biographer, with the possible exception of the depiction of Joseph Chamberlain, whose train of thought James finds easy to follow (p. 206) but who will nevertheless leave many observers suspicious. The vignettes are set in a broader understanding than this suggests, even if the flavor of it is occasionally overripe. ("Disillusion and bitterness stalked the land on the morrow of victory" [p. 457].) It is very much history from the heart.

One effect of this depth of emotion, however, is an almost Whig tendency towards moral pronouncement and this sometimes interferes with what Rhodes James hopes to achieve. It is no part of the brief of a professional historian to report that F. E. Smith was "tragically flawed" (p. 232), or that Lloyd George's private life "left much to be desired" (p. 323), or to suggest that Sir Frederick Maurice's intervention in politics in 1918 was "intolerable" (p. 387). Similarly, one wonders about the insistence that Baldwin is "a refreshing personality" because of his "humanity and basic moral standards" (p. 490) or the contention that Cunliffe-Lister (on whose biography James is currently engaged) "deserves all praise" (p. 547). This latter concern with Lord Swinton may have contributed to the otherwise mystifying statement that the Parliament of 1931–40 was the most brilliant and passionate of the century.

Useful perspectives in so broad a study as this are inevitably linked to the parts of the narrative which the author knows best; and it is plain from Rhodes James' list of sources that this book rests to some extent on his previous writings. Yet it seems arbitrary to carp: no author could measure a period of this breadth with an exactly even tread.

Furthermore, at the level of tangible fact James rarely puts a foot wrong. An overall sense of balance within the treatment (presumably the author's most enduring headache) is beautifully achieved, and the odd mechanism which makes Seeley's *Expansion of England* and Hobson's *Imperialism* appear two years too early is noticeable only because of its singularity.

For all that, Winston Churchill, whom Rhodes James continues to dislike, might have complained that the book was without a theme. Perhaps what it shows is that the real British revolution lay in the degree to which one did not occur.

MICHAEL BENTLEY
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RAYMOND CHALLINOR. *The Origins of British Bolshevism*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 291. \$16.00.

Raymond Challinor argues that sectarianism was the effect, not the cause, of the decline of the Socialist Labour Party (SLP) in Britain after the First World War. Political parties which are isolated from the mainstream of politics turn toward sectarianism. After 1920 the counterrevolutionary ideologies of social democracy and Stalinism imposed isolation upon British revolutionary groups. But Challinor refuses to acknowledge that the emphasis on establishing Marxian doctrinal purity, i.e., revolutionary socialism, neutralized the SLP. The author's objective is to explain why revolutionary socialism failed to conquer Britain before 1920, while the "class collaboration" of the trade-union leadership and the parliamentary Labour Party laid the foundations for later success. His less than satisfying answer is that "the gradualist approach was the traditional one since the decline of Chartism" (p. 282).

Challinor confines his discussion of the origins of British bolshevism to the years 1900-22. He discusses the SLP's formation, its early struggle for survival both within the Left and in pre-World War I society, its role in the labor agitation of the war years, its postwar opportunities, its treatment at the hands of Lenin, and its legacy to today's Left in Britain. He relies upon published materials for his sources. The movement's newspapers and journals, contemporary pamphlets and books, and recent historical studies are consulted for the narrative. References to manuscript sources are scant, although the use of official government documents tempts speculation about their full potential. Unfortunately, the book lacks a preface and contains no bibliography; a list of abbreviations for the numerous political parties would also have been helpful.

From these sources several themes emerge. The SLP's program emphasized the revolutionary struggle for industrial democracy. But unlike the Russian Bolsheviks, the SLP applied its revolutionary ardor to raising class consciousness, not overthrowing the government. Whatever effectiveness it could have enjoyed was squandered in negative rhetoric aimed at establishing itself as a pure revolutionary socialist party and at discrediting others on the Left as class collaborators or "backsliders." Despite Challinor's disclaimers, the SLP drew its rank-and-file strength primarily from the militants in the Clydeside; its membership was never large, though always growing. Its success in creating an industrial democracy depended upon the shop stewards, who emerged as the leading labor agitators of their time, but who were also the most vulnerable. The SLP did not benefit from the Bolshevik success in Russia. The blame, Challinor writes, was Lenin's because he mistook the class collaborationists in the British Communist Party and the parliamentary Labour Party for the real revolutionaries in Britain and failed to support the SLP. Consequently, reformism prevailed over revolution. The demise of the SLP in 1921 ushered in "the Ice Age of British socialism" (p. 275).

This is traditional labor history, but with a different twist. Challinor shifts the focus from the trade union and parliamentary Labour Party leaders to the secondary level sectarian leadership. For Marxists of the 1970s, Challinor rescues the SLP from the obscurity of failure in order to stress the relevance of its principles for current revolutionary socialism. For historians, his examples of the government's employment of police detectives, "spies," and *agents provocateurs* serve as a reminder of the effectiveness of social control mechanisms in dealing with visible political dissidents. This study contains sound advice for today's revolutionary socialists in Britain, but historians may find it excessively polemical.

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PAUL-WOLFGANG HERRMANN. *Die Communist Party of Great Britain: Untersuchungen zur geschichtlichen Entwicklung, Organisation, Ideologie und Politik der CPGB von 1920-1970*. (Marburger Abhandlungen zur Politischen Wissenschaft, number 31.) Meisenheim am Glan: Verlag Anton Hain. 1976. Pp. 448.

Paul-Wolfgang Herrmann has chosen to write on the historical development and politics, the organization and ideology of the British Communist Party from 1920 to 1970. No post-1970 material is cited in notes or bibliography, and some internal evidence points to composition around 1970. The

first quarter century receives brief treatment so that the author can concentrate on the period after 1945. For the earlier period, he is willing to leave the reader to James Klugmann, Leslie J. Macfarlane, and Henry Pelling. His sources are leftist and party journals, and other party publications as well as a range of primary and secondary literature. There is no evidence that the author used the Butler-Nuffield studies of each of the postwar elections.

His thesis is fairly complex. The party was founded with false expectations about imminently developing revolution. Thereafter the party passed under Russian control, that is, was bolshevized, a change that appears to have been less violent in Britain than, for example, in the United States and France, although Herrmann does not discuss the matter. The party was widely seen as being under foreign control. Organizationally it imposed a democratic centralism at odds with British traditions in which the workers shared.

The inflexible party, nevertheless, gradually ceased to be revolutionary and in elections and propaganda became reformist. Indeed, Herrmann makes the startling statement that the Soviet Union, formerly center of world revolution, has become today the center of reform Communism (p. 409). The statement is startling to the reviewer, who believes that the Soviet Union and British and American Communists have often frustrated radicalism and reform.

The party has attempted to influence, even to join, the Labour Party, and its achievement has been so slight that years ago Labour's radical *Tribune* urged that, in the interest of advancing socialism, the Communist Party dissolve itself. But it usually has key positions in the trade-union movement, and some party trade unionists have shown a leadership potential lacking in the party chiefs. If it often appeared foreign, the party also acquired an English side. Many of the rank and file, Herrmann thinks, are democrats, and at second hand (there is far too much of this) he quotes Arthur Koestler's epigram that the English Communists are more like the Pickwick Club than the Comintern. The remark was made long ago and has always been misleading. There is nothing gently clumsy about Kim Philby and Guy Burgess. Clearly, but regrettably, the subject of espionage is excluded from Herrmann's intent.

Herrmann has written a serious and thoughtful book on a movement whose ultimate ineffectiveness and political meaninglessness form a consistent feature of quoted documents and commentary. The first section, on politics and conferences, provides subject matter that is sometimes very difficult to read, compounding jargon and irrelevance to glaze the eyes. The latter sections

are far more interesting, notably the discussion of Kenneth Newton's *The Sociology of British Communism*, the party and students, the trade unions, and the guidance and assistance of the British Communist Party to the Communist Party of India.

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WILLIAM D. MULLER. *The Kept Men: The First Century of Trade Union Representation in the British House of Commons, 1874-1975*. Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. xx, 283. \$21.50.

A Conservative member of Parliament in 1954 labelled as "kept men" all his fellow MP's who received money from outside sources. Between thirty and forty percent of the Labour members of recent Parliaments have been such "kept men," receiving from their trade-union sponsors an annual grant for their constituency parties, up to eighty percent of their election costs, and sometimes a small annual fee for themselves. Trade union sponsorship began in 1874 when two mining union officials were elected to the House of Commons under the Liberal label. These Lib-Labs eventually reached a total of fifteen; but the Labour Party started in 1900 and by 1908 it absorbed the last of the Lib-Labs. Before 1911 MP's received no government salary; and so both the Lib-Labs and most of the early Labour MP's were totally dependent on their union payments. They were not merely subsidized as are their successors today; they were totally kept.

William D. Muller, a political scientist, has produced an excellent study of the trade-union-sponsored MP's, based on extensive reading and many interviews and replete with thirteen tables. He tells us that unions once used sponsorship as a form of honorable retirement for unwanted officials, but do so no longer. Sponsored MP's, he demonstrates, speak frequently on bills directly affecting their unions; but on all other issues they speak less than other MP's, ask fewer parliamentary questions, and occupy far fewer positions in governments or in shadow cabinets. They are, however, overrepresented in the Whip's Office, and have been much more loyal to the leadership in internal party battles. Other writers have said these things, but with less detail and with fewer statistics. *The Kept Men* is a useful book for all specialists in British trade-union and Labour Party history and in the recent workings of the House of Commons.

Muller tries, but fails, to find a distinctive role for the sponsored MP's today. The trade unions usually bypass them in dealing with governments; and even within the Labour Party, the unions and

their MP's often go their own ways. In the party battles over unilateral nuclear disarmament in 1960 and 1961 and over prices and incomes policy from 1964 to 1970, almost as many sponsored MP's voted against their unions as voted with them. The unions influence Labour Party policy through their bloc vote at the annual conference and through their direct representatives on the National Executive Committee, but not through their sponsored MP's. Sponsorship has sometimes protected that increasingly threatened species, the Labour MP of working-class origin; but even here Muller notes a disquieting trend. Professional and technical unions are sponsoring middle-class MP's and even such a proletarian union as the Transport and General Workers Union has middle-class branches which aspiring Labour politicians are eager to join. "The twenty-seven TGWU MP's elected in 1966 included three teachers, two solicitors, a barrister, an economist, and three journalists. Only six MP's might be said to approximate to the traditional 'cloth cap' image of working class representatives . . ." (p. 187). This suggests that union sponsorship has lost its former functions, but remains as one of the creaky gears which help keep the complicated Labour Party machine in motion.

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SHEILA LEWENHAK. *Women and Trade Unions: An Outline History of Women in the British Trade Union Movement*. New York: St. Martin's Press. 1977. Pp. xi, 308. \$14.95.

Proposing to explain why women's trade-union organization in Britain has been weaker than men's, and why its strength has fluctuated, Sheila Lewenhak in fact accomplishes a more limited but still useful objective. The best and most original part of the book is the post-1920 history of the efforts to represent and recruit women and the responses to women's issues within the institution of organized labor in Britain. Although the Women's Trade Union League dissolved into the Trades Union Congress (TUC) in 1921, the problems of organizing women workers by no means disappeared. Debate between proponents of special representation for women and partisans of full integration in trade-union institutions continued in the 1920s, while female membership fell steadily along with the more general decline in trade-union numbers. Concern over losses led in the early 1930s to a renewed interest in special efforts to recruit and represent women. The first TUC Conference

of Unions Enrolling Women, which met in 1931, created a permanent National Women's Advisory Committee (NWAC). By the time the equal pay campaigns began in the mid-1930s, however, NWAC had become a captive of the labor establishment and incapable of independently voicing women's concerns. Spurred by recovery, rearmament, and the wartime mobilization of labor, female trade-union membership climbed again after 1938. The diffident leadership of NWAC was challenged by a new generation of more militant women who refused to go along with the Labour Party's wage freeze after 1945. For the next fifteen years they pursued an unpublicized campaign for equal pay, which met with some success in the public services and finally came into the open after 1963. The broader antidiscrimination legislation of the 1970s, however, followed only after changes outside and inside the trade-union movement built up pressure on the Labour Party. The growth of pressure groups and public support for women's equality coincided with the trade unions' own restlessness under Labour's restraints. Lewenhak is guardedly optimistic that the resulting alliance in favor of women's equality will last.

The outline approach and extension of the original dissertation to include the pre-1920 period detract from the coherence of the study without adding much that is original. The early chapters say little more than that women were part of organized labor from its beginnings. The discussion of the philanthropic women's trade union organizations, which developed after the 1870s, suffers as a result of its construction around personalities, the "series of individual women of great strength of character" (p. 67) who dominated the WTUL, the Scottish League, and the National Federation of Women Workers. Rather than stretch the scope of the book, Lewenhak would have done better to refine and elaborate the most interesting themes of its second half. The question of special representation versus full integration of women in the labor movement deserves more development. In a context where some women opposed equal pay because they feared it would keep them out of jobs, and some men supported it for the same reason, the complexities of the equal pay campaign provide rich material for further analysis. Finally, we need a better understanding not just of the men's responses, but of the sources of women's initiatives inside the trade-union movement.

Since so little is currently available on this topic, it was probably too difficult to resist the temptation to cover the whole ground. Older studies like *Women in Trade Unions* (1920) by Barbara Drake or *Women in the Trade Union Movement* (TUC, 1955) are out of print and hard to find. This volume, with its useful bibliography, makes the information more

accessible, and contributes especially to our knowledge of the period after 1920.

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JOHN P. MACKINTOSH, editor. *British Prime Ministers in the Twentieth Century*. Volume 1, *Balfour to Chamberlain*. New York: St. Martin's Press. 1977. Pp. 282. \$14.95.

These studies of British prime ministers from Arthur Balfour to Neville Chamberlain place particular emphasis on three aspects of their leadership: their handling of the Whitehall machine, their dealings with their own party inside the House of Commons and in the country, and their overall management of Parliament. If any general conclusion emerges, it is simply that each prime minister imposed his personality and tone on his cabinets and administrations.

The essays on Balfour, Henry Campbell-Bannerman, Ramsay MacDonald, and Stanley Baldwin plough well-worn furrows. Peter Fraser recognizes the accomplishments that resulted from Balfour's freedom in matters of foreign policy and defense, but notes how he supported the rear-guard action of a vanishing patrician class in matters of social policy and legislation. José F. Harris and Cameron Hazelhurst agree that Campbell-Bannerman was not a firm leader in cabinet or in the country; yet they note how he managed to mediate some of the fiercer antagonisms within the Liberal Party. Trevor Lloyd concludes conventionally that MacDonald never worked out a way to translate his long-term vision of the future into the details of day-to-day political activity. And John Campbell, rejecting some of the more sanguine rehabilitations of Baldwin's performance, sums him up as a thoroughly decent man, full of vague goodwill, but "possessed of utterly conservative instincts, a good deal of self-satisfaction and no ideas—temperamentally quite unfitted for the job to which he had never aspired . . ." (p. 216).

The major essays, as one might expect, deal with Herbert Henry Asquith, David Lloyd George, and Neville Chamberlain. Cameron Hazelhurst's balanced inquiry into Asquith's leadership offers no pat formulations or stereotyped conclusions. Recognizing the frequent tendency to contrast Asquith's six years of success in peacetime with his more troubled wartime prime ministership, he nevertheless concludes that Asquith in fact is open to criticism for his administration in the earlier years. To be sure, he overcame many crises, but some were of his own making. Even today, after sixty years, the verdict appears still to be out.

As for Lloyd George, Kenneth O. Morgan

rightly emphasizes the tension between his instinctive populism and his urge for power. During his wartime leadership, he used power with great artistry, marshalling the cabinet secretariat and the Garden Suburb to help dominate his government from within and the Imperial Cabinet to do likewise from without. But as the head of a coalition he was not nearly so sure of his control of Parliament or his influence on the press. Constantly seeking to construct a new political base for himself, he managed to survive in the immediate post-war period, but as peacetime politics returned his Olympian position could not be maintained. As relations with the Unionist majority in Parliament deteriorated, so too did Lloyd George's hope of turning the premiership into a kind of strong presidency. His feat of equilibrium could not survive indefinitely. As Morgan puts it, the greatest show on earth came to an end.

By far the most controversial essay is Alan Beattie's well-argued defense of Neville Chamberlain as a strong and effective prime minister. Contrasting Chamberlain's "genuine political character and style" with the brilliant facade of the political jerry-builder, Lloyd George, Beattie notes how Chamberlain attempted to create a coherent foreign policy out of a confused range of alternatives. He may have failed, but he recognized that politics requires choice, and he had the reason and moral vision, declares Beattie, that were inseparable from the policies he pursued. Chamberlain clearly was not the fool nor the villain of earlier mythology, but to portray him in Beattie's almost hyperbolic terms does not really come off. The crabbed narrowness of Chamberlain's personality made certain that this honorable man saw all non-conservative opposition as blind, stupid, and almost malign. Words like "hate," "despise," and "infuriate" that dot Chamberlain's references to Liberal and Labour critics hardly bear out the picture of a noble personality selflessly staking his reputation on a course he knew to be unpopular. Beattie himself constantly differentiates between the "moralist" and the "historian." In effect, he argues that the "moralist"—a bad word in these days when many scholars abdicate all responsibility—is a "historian" who disagrees with Beattie. Yet whatever criticism this essay on Chamberlain evokes, it is nevertheless the most challenging in a generally effective group of thoughtful and intelligent studies of leadership in twentieth-century British politics.

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ANTHONY JOHN TRYTHALL. *"Boney" Fuller: Soldier, Strategist, and Writer, 1878-1966*. New Brunswick,

N.J.: Rutgers University Press. 1977. Pp. xiii, 314. \$13.95.

Despite his many historical writings, Fuller will probably be best known to readers of the *AHR* as theoretician of armored warfare and fascist sympathizer. The author of this fine biography takes a longer perspective. The order of emphasis is well indicated by the subtitles; Fuller emerges as an authentic military intellectual with career frustrations and a continuing need to seek other outlets for his energy.

Anthony John Trythall brings exceptional qualifications to his task. He is himself an army career man. This book also shows him to be a good judge of character, possessed of common sense, and a perceptive, if occasionally repetitious, stylist. *Mutatis mutandis*, one is curiously reminded of a novel by Trollope, from the clerical background through the sharp-tongued wife to the close in Tunbridge Wells.

In the foreground, of course, are some very un-Trollopean themes, above all that of war. Fuller was indeed a militarist, contemptuous of democracy, glorifier of war, not averse to violence. Once enrolled in the British Union of Fascists, he fully shared and expressed that movement's anti-Semitism. He was also a continuing "student of the occult," with three works on the subject on his publications list.

Trythall makes this unusual life-journey comprehensible. The apex of Fuller's career was the decade between his appointment, in his late thirties, to the Headquarters Heavy Branch of the Machine Gun Corps in France at the beginning of 1917 to his refusal to assume command of the Experimental Mechanized Force. It was Fuller's supreme opportunity to put his ideas into practice, and he threw it away on quite trivial, and remediable, administrative grounds. After this act of defiance his career, despite promotion to general officer rank, languished. "Bombay, had he gone there as a Major-General when the post was offered in 1931, would have been the ultimate in hot Siberias" (p. 145). Instead, he went back to half-pay and then retirement, joined the fascists, and wrote historical and theoretical works on military affairs.

Trythall devotes much space to an analysis of the more technical (and more valuable) aspects of Fuller's earlier military thought. An example is the concept of "strategic paralysis," a major feature of Fuller's plan for the unfought 1919 campaign, the main lines of which memorandum were approved by Marshal Foch. The enemy command structure was to have been attacked before the major assault on the front line, indeed an original approach. On the long struggle between tanks and other arms for

supremacy within the armed services, the author comments that "there are still separate arms in most major armies but they nearly all travel, and generally fight, in vehicles which look very like tanks" (p. 268).

This book is well worth reading.

PAUL GUINN

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PETER LOWE. *Great Britain and the Origins of the Pacific War: A Study of British Policy in East Asia, 1937-1941*. New York: Oxford University Press. 1977. Pp. ix, 318. \$19.50.

In the 1960s British scholars in their investigation of the origins of the Second World War moved away from an almost exclusively European concern to consider a wider sphere. Britain's relations with the Far East have received particular attention. Peter Lowe's study of British policy in East Asia, 1937-41, is only the latest in a series of monographs in this field. The premise from which Lowe starts is that British government and society in the 1930s showed insufficient understanding of Japan. Apart from a few astute observers the British policy-makers were convinced that Japan could not fight a large-scale war for a prolonged period and that, if it attempted to do so, such a conflict would prove too great a strain for the Japanese economy. At the same time, Japan could not afford to retreat from the policy of expansion, as that would be a betrayal of its values.

Lowe points to the mistaken belief of British and the United States officials that if war came to the Pacific Japan could be contained without undue difficulty. Churchill, in particular, opposed concessions to Japan, and in this was supported by the Foreign Office and even the chiefs of staff, who felt that Japan could be held. According to Lowe, Britain did not pursue a policy of appeasement toward Japan, apart from a few individual instances carried out under pressure, such as the customs agreement of May 1938, the Craigie-Arita formula over Tientsin in July 1939, and the temporary closure of the Burma road in July 1940. The reason adduced for this is that Britain did not believe that Japanese expansion beyond Manchuria was reasonable, or that concessions would satisfy Japan. British policy also had to obviate United States antagonism, as that country would have to play the chief role in any conflict.

Lowe's scholarship is of variable quality. In the section dealing with the events leading up to the outbreak of the European war he relies heavily on a selection of published works, the *Documents on British Foreign Policy*, and only those sections of the

Foreign Office papers dealing with China and Japan. His neglect of the Foreign Office correspondence with the United States on the Far Eastern crisis has led to a slightly distorted account of British policy in the Far East, and particularly of the role of Eden. Eden, and Foreign Office officials like Gladwyn Jebb, were anxious to secure "joint" action with the United States in the area, so that if the United States were involved alongside Britain in a war with Japan, it would automatically become Britain's ally in the event of war in Europe. Eden tried to conceal this policy from Chamberlain, the relevant documents being marked not for circulation to the prime minister or the Cabinet. As Lowe is anxious to revive Craigie's reputation he might have pointed out that Craigie was trying to pursue a policy in line with that of the State Department rather than that of the Foreign Office: Craigie made this clear to the United States en route to taking up his appointment as ambassador. Lowe's account of Britain's relations with Australia over defense questions in the Far East omits certain crucial conversations between the dominions secretary and the high commissioner. Indeed, Lowe's almost exclusive reliance on diplomatic documentation gives, perhaps, a misleading impression of Britain's position with regard to the dominions.

The analysis in the latter half of the book, however, is perceptive, and the use of documentation meticulous. It is here that the considerable value of this work lies. There is a detailed account of the views and influence of officials like R. A. Butler and Sir John Brenan, of debates within the Foreign Office and the Cabinet, and a vindication almost throughout of the stand taken by Craigie and Sir George Sansom. The extent to which Britain had to consider the views of the United States on issues such as the closure of the Burma road and the trade embargo against Japan is carefully weighed.

Lowe offers an outstanding study of the making of British foreign policy during the early 1940s. In this section of the book an overall perspective is achieved which should widen its appeal beyond the circle of Far Eastern specialists.

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DAVID FITZPATRICK. *Politics and Irish Life, 1913-21: Provincial Experience of War and Revolution*. Dublin: Gill and Macmillan. 1977. Pp. xx, 394. £15.00.

In this study of politics and society in County Clare during the Irish Revolution, the Australian-born historian David Fitzpatrick contends that lo-

cal influences determined the course of the revolutionary movement. While this is not a novel interpretation, the author bases it on prodigious research in primary sources that range from official archives in Dublin and London to local records, private papers, and interviews with survivors of the period. Almost one hundred pages of notes and bibliography, together with a series of special maps and statistical tables, attest eloquently to his scholarly diligence.

The text is divided into two parts, the first of which relates the experience of the "Old Order"—crown forces, Unionists, and Home Rulers—before, during, and after the 1914-18 war. Part two discusses the "New Politics" of Sinn Feiners, revolutionary administrators, and guerrilla fighters from the 1916 Easter Rising to the Anglo-Irish truce of 1921, with a closing chapter which examines the relationship of separatism to social change. Although the problems of the crown forces during "the Troubles" are a familiar story, treating them in a local context adds detail and interest. The same is true of the plight of Clare's tiny Protestant community in the twilight of its ascendancy. While Fitzpatrick readily concedes the rapid decline of Home Rule organizations after the Rising, he also shows that prominent Redmondites who underwent a timely conversion to Sinn Fein retained local power for several more years. Indeed, the book's most important point is how much Sinn Fein as a mass movement owed the Old Party, in terms of political organization and approach as well as recruits. Yet despite its remarkable growth, provincial Sinn Fein enjoyed only a brief existence. Lacking a definite program after its impressive victories in 1918, the party drifted, and its local leaders were pushed aside by extremists as the struggle with Britain intensified in 1920. This militant elite not only disrupted British administration by guerrilla warfare, but it also gave the Republican government a semblance of reality in the provinces by operating courts and local government in defiance of Dublin Castle. Closely following the thread of parochial concerns and personal rivalries that runs throughout his narrative, the author confirms the importance of local initiative and autonomy in the military campaign against the British. Finally, he explores the interaction of labor, the farming community, and the Republican government, showing how the government's concern for national unity and need for farmers' support helped defeat labor's efforts for social change.

Fitzpatrick makes a strong case for the supremacy of localism and conservatism in the revolutionary movement, but his conclusions do raise some questions. Does not such heavy emphasis on local influences tend to obscure the importance of cen-

tral initiatives in matters like publicity, finance, intelligence, and, of course, the Republican commitment? And were the separatists really as politically conservative as the author claims? Political experiment was hardly feasible during the struggle with Britain, and the civil war that followed put a premium on institutional stability and fiscal caution. Even so, the Free State made some interesting innovations in government and might have tried others had conditions been more settled. On the other hand, was radical social change ever a real possibility, given the underlying stability of Irish society and the social conservatism of all groups and classes, including labor itself? Finally, one must ask just how typical was Clare's experience in the revolution?

Such questions, however, do not materially diminish Fitzpatrick's achievement. By putting County Clare under a microscope, he has added another dimension to the study of the Irish revolution, enriching our understanding of both its local and national political development. Every student of modern Irish history should be grateful for his pioneering work; may it inspire some of them to emulation.

JOSEPH M. CURRAN
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JACQUES BEAUROY. *Vin et société à Bergerac: Du Moyen Age aux temps modernes*. (Stanford French and Italian Studies, number 4.) Saratoga, Calif.: Anna Libri, for Department of French and Italian, Stanford University. 1976. Pp. 293. \$14.50.

Jacques Beauroy's book is an important contribution to the history of viti-viniculture, a field that is growing more rapidly in France than in other wine countries and that has hardly made an appearance in the United States. Beauroy enjoys at least two advantages over many other scholars in this area: he is a well-trained historian and, more rare, he comes from a vinegrowing background. His study, therefore, combines information about the economic and social aspects of the industry, which is standard for most works in the field, and also technical information, which rarely emerges in most wine books.

Beauroy's book indicates that wine production in the Bergeracois followed a pattern of growth similar to those of most wine districts: clerical and lay nobles laid its basis from the ninth to twelfth centuries; then with the renaissance of town life in the twelfth century, urban bourgeois expanded the vineyards as wine became a profitable commodity of local and foreign trade. Like Roger Dion, the great pioneer in the field, Beauroy argues that

townsmen transformed wine production in order to enhance quality and expand sales. Their activity depended on the Dordogne River—for access to the Atlantic Ocean—and on the Dutch, who constituted the major market for their dry red and semisweet white wines. This trade reached a high point between the mid-seventeenth and mid-eighteenth centuries when a drastic decline occurred, leaving the merchants and vine growers of Bergerac in an economic crisis. At this point, Beauroy ends his study.

Given the long period his work covers, Beauroy is at times necessarily general, even vague; his overly brief explanation for the decline of trade with the Dutch leaves many questions unanswered. A more serious deficiency is his failure to answer a question he himself raises: what was different or unique about the wine economy of Bergerac? After reading him we know that its system of labor contract was certainly different from the *vigneronnage* of the Beaujolais, the fixed payment common to Bordeaux, and the crop sharing of Burgundy. There were, perhaps, other distinctions. We also are aware that the sparsity or absence of documents seriously hinders a fuller study of important issues. For this reason, it serves little purpose to dwell on lacunae, since his book overall is a very good one, offering considerable information gleaned from public and private archives and many useful insights.

The sections on the living standards of the *vignerons* (vine trimmers) are enlightening; his quantitative data will be useful to social historians, as will his views on the food riots beginning in 1773. The discovery of family records also has enabled him to explain the viticultural practices that led to the rising quality of wine by the seventeenth century. Equally important are the numerous maps that locate vineyards and the frequency tables that explain economic and social developments. For these reasons I heartily recommend *Vin et société à Bergerac*.

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JEAN-CLAUDE PERROT. *Genèse d'une ville moderne: Caen au XVIII^e siècle*. Volume 2. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 44.) Paris: Mouton. 1975. Pp. 553-1157.

Jean-Claude Perrot's two-volume study of Caen, the capital of Lower Normandy, is a welcome addition to the scarce but growing literature on the cities of the *ancien régime*. It nicely complements

two recent publications, Maurice Garden's *Lyon et les Lyonnais au XVIII^e siècle* (1975) and Marcel Courdurié's *La dette des collectivités publiques de Marseille au XVIII^e siècle* (1974), which admittedly is narrower in scope.

Although the second volume is the focus of this review, the two must be read together, since only volume one contains any introductory material. The first volume also explains the documents that Perrot used and looks in detail at such factors as the uses of the land, the role of the market, and the mutual dependence of city and countryside. Industrial production in Caen, particularly in the textile sector, is examined, as are the methods of exchange, which included the fairs of Caen and Guibray.

Building on the socioeconomic base established in the first part, volume two is concerned with the daily life of Caen—the people, the institutions, and what Perrot calls the “opponents of change” (p. 561)—along with the divisions within the population, densities in different sections of the city, new construction, in- and out-migration, and internal transportation and communications. Two lengthy concluding chapters are devoted to analyzing the movements of prices, salaries, and *rentes*, and to a detailed examination of the “raw materials” of historical demography—births, marriages, and deaths. Perrot has considerable evidence of the rather widespread use of contraceptive methods in the eighteenth century. In contrast to recent studies on rural Norman populations such as those of Crulai and the Beauvaisis, where prenuptial conceptions were rare, Perrot notes that at Caen “the phenomenon is already widely accepted within the urban tradition by 1650” (p. 875).

The 100 pages of appendixes (all of which are in the second volume) and the 64 illustrations (of which 33 are in volume two) are a most impressive part of Perrot's work. The illustrations include six different contemporary views of Caen and its chateau in the seventeenth and eighteenth centuries. For the statistician, demographer, or economist, the tables constitute an almost limitless source of data. For example, one table charts marriages, conceptions, and deaths by months and correlates the resulting graphs with the price of wheat.

Perrot's *Caen* is a very valuable addition to other studies of urbanization and city growth in the eighteenth century. His mastery of the sources is matched by his familiarity with the streets, apartments, squares, and *quartiers* of the city. The depth of the prerevolutionary study is not carried into the 1790s, despite the title of the book. This is, however, a small point, and is more than balanced by the thoroughness with which Perrot has examined Caen prior to 1789. It should become the definitive work on the capital of *Basse-Normandie* and in-

dicates the direction in which similar urban studies should move.

THOMAS F. SHEPPARD
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LOUIS TRENARD, editor. *Histoire d'une métropole: Lille, Roubaix, Tourcoing*. (Univers de la France et des pays francophones, number 37.) Toulouse: Privat. 1977. Pp. 520.

The Toulouse publishing house of Privat has commissioned a series of original, single-volume histories of cities in French-speaking Europe to complement its successful series on the history of French provinces. Of course the volumes on the provinces make ample reference to urban developments, just as the city histories tend to describe region-wide phenomena, especially for the modern period. But of all the volumes Privat has published, only this one, *Histoire d'une métropole: Lille, Roubaix, Tourcoing*, is explicitly focused upon urban growth as a problem in the formation of a region. The fortunes of these three cities, the authors assert, can only be understood in terms of how each one has interacted with the other two and with the rural districts of Flanders. This assertion is not original, but it is rarely worked out in analysis as completely as in this book.

With careful attention to the limits of documentation the authors explain and describe the forms of regional life that prevailed in antiquity and in medieval times. By the mid-thirteenth century life in Lille—a city without Roman antecedents—began to affect the structure of rural civilization. Lille grew on a money economy by functionally differentiating itself from the countryside without becoming independent of it. From the late Middle Ages until the seventeenth century Lille and Brussels served as the centers of political and social life in Burgundian Flanders. Indeed, for this period, the history of Lille resembles the evolution of cities throughout Flanders. In the chapters on the medieval and early modern periods, there are many references made to well-documented topics that await scholarly attention and to issues that contribute to a general understanding of regional development. Did Lille belong to a Flemish network of cities? How did the oligarchy of Lille become a *noblesse de robe*? Why did Lille never become the center of a state? These and other subjects are discussed in ways that allow the authors to analyze city-county interaction in the Lille region.

From the seventeenth century on the amount of documentation and the number of historians using it increase enormously. Yet questions about regional development are handled with less clarity in

the chapters dealing with the modern period than in the earlier chapters (except in the last chapter, which is devoted to the region's contemporary life). In part this is because the authors lacked enough space to insert the necessary analytical apparatus. Thus there is a page on the lack of a local press in the eighteenth century, but nothing on the circulation of information between Lille and the other units of its region; similarly, the rise of rural incomes in that century is not linked to changes in the city's economy. With the rise of the mechanized textile industry in the nineteenth century, Roubaix and Tourcoing grew, and the region took on its modern shape as a metropolis without a center. The industrial, religious, and political factors in change are all given full treatment, but one is left to guess what combination of factors made this region so different from other regions in France in its capacity to support heavy industry. Only in the last chapter is there a discussion about whether Lille's relative lack of dominance is a strength or weakness in the region. Perhaps part of the problem in analysis lies in the fact that Lille and its region continued to function as part of a Flemish-Belgian zone long after questions about political allegiance and sovereignty were settled in the reign of Louis XIV, yet the political lines compel the authors to study this region as a part of France.

This book supports two views that are often held to be contradictory: it sustains the opinion that the historical reconstruction of pre-1800 urban phenomena is critical to the analysis of recent developments; yet it also confirms the chronology which dates truly modern metropolitan growth only from the early nineteenth century. It is a useful source of information about an important region and about the critical issues urban regional history involves.

JOSEF KONVITZ

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MARCEL BERNOS *et al.* *Histoire d'Aix-en-Provence*. Aix-en-Provence: Edisud. 1977. Pp. 392. 75 fr.

Urban history is not inevitably the history of economic growth and industrial strife. That is one reason why sociologists have coined the term "urban village." The institutions that did most to shape the two-thousand-year history of Aix-en-Provence were neither commercial nor industrial, but rather administrative and ecclesiastical. From its founding by Sextius Calvinus in 124 B.C., Aix was an administrative center; its first bishop appeared in 375 A.D. and it became a provincial capital in Roman Gaul under Diocletian. Sources are unusually scarce for most of the Middle Ages, but at least from the "Time of Troubles" between 1350

and 1400 until 1789, it is clear that the city remained firmly in the hands of its robe magistrates and clergy. Indeed a good case can be made that the law and the Church remained the chief pillars of society until the end of the nineteenth century and into the twentieth.

The eight authors of this collective undertaking (all but one from the University of Provence or Aix-Marseille) have managed to give unity to this history, despite approaching it through their own specialties. They demonstrate that urban history need not be linear and that cities like Aix underwent cycles of expansion and decline, periods of population growth, religious fervor, cultural effervescence, and urban planning followed by epidemic, famine, war, and depopulation, to be succeeded again by a revival of local energies and creative élan. For Aix, its most dynamic centuries were the thirteenth and the seventeenth, its most difficult, the fourteenth and sixteenth, and its most moribund, the nineteenth. Aix reached its apogee sometime between 1660 and 1720 when political order and the fervor of the Catholic Reformation combined with the enlightened leadership of the parliamentary magistrates to give Aix the aristocratic beauty it still preserves in its architecture and public monuments as well as the bounty of its new charitable, educational, and cultural institutions.

This "civilization" depended on the city's status as a provincial capital, not as a center of economic activity, no matter what "potential" its geographic position promised for commercial exchange. It was the seat of the royal governor and the intendant, of a parlement, a bishopric, and a vigorous municipal government. As in the Bayeux of Olwen Hufton and the Toulouse of Philippe Wolff, the economic and psychological well-being of the entire social structure depended on the prestige and capacity of a narrow elite of notables. The social profile of Aix in 1695 reveals this dependence and also serves as a model of what a preindustrial city was like—one-fifth peasants, one-fifth house-servants and coachmen, one-fifth *rentiers* and widows, one-fifth lawyers, officials, and clergy, and one-fifth "in trade" (and most of that for local consumption). Even more telling is the fact that over a century later, in 1835, the population of Aix was unchanged at thirty thousand inhabitants and its social profile substantially the same, except that the old officialdom had become a bureaucracy, the domestic services were more feminine (80 percent), and the rural component even larger (23.6 percent).

Little wonder the authors are tempted to identify the determinants of long-run continuities rather than list the "causes" of short-run "ruptures" or crises. Here in the long history of a provincial town, political and legal changes—even

those of a decade of revolution—do seem like surface waves on the tides of gradual demographic, economic, social, and cultural mutation. Aix-en-Provence “missed” the entire nineteenth century, despite vain attempts to lure a rail line, launch a copper industry, or keep its young men from emigrating to Marseille and elsewhere. Somehow the town’s notables—magistrates, lawyers, clergy, landlords, and *rentiers*—survived every crisis, if “crises” they were, for half a millenium or more. Only since 1950 can one detect a decided demographic and urban mutation as Aix becomes part of a new, larger *région marseillaise*. Now only some aging notables and a few sentimental intellectuals will continue to hold on to the mythology of “good king René,” the last count of Provence, and to the fountains and plane trees along the *Cours Mirabeau*.

ROBERT FORSTER
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YVES CAZAUX. *Henri IV ou la grande victoire*. Paris: Albin Michel. 1977. Pp. 488.

I found this a difficult book to review, for my reaction to it was at first quite negative. It is a popular study with many of the faults of such works—ample use of exclamation marks, ad hoc remarks, and so on. Nevertheless, the faults (as I perceived them) came to be overshadowed by the value of the work, which should not be quickly rejected.

Yves Cazaux has selected as his topic the atmosphere and events which surrounded the successful accession of Henry of Navarre to the French throne. He paints the background in an introductory section devoted to the years 1584–85. Skipping from there to 1589 he deals successively with various social forces (e.g., the League, the Sixteen, the Press), the first few years following the assassination of Henry III, and the final “great victory” of Henry IV (culminating with the coronation at Chartres, the entry into Paris, and the postscript which was papal absolution). Cazaux treats his material topically rather than chronologically, and he manages continually to build up the tension inherent in his topic. Some readers will find useful the several appendixes, which include a chronology and numerous genealogies. In addition, Cazaux appended the discourse pronounced by Antoine Loisel before the Parlement of Paris immediately after the king’s entry; it is from this discourse that Cazaux obtains part of his book’s title.

Although the study is a popular one (for example, it is not lavishly annotated, despite the 38 pages of notes for 369 pages of text), it cannot be taken lightly. Cazaux has published numerous

works on the sixteenth century, and he knows his subject well. His bibliography (33 pages) of manuscript and printed sources is impressive, but it and the book would have been better if Cazaux had not neglected all of the literature in German and most of the recent literature in English. There is much in the book with which one can disagree, but, I suspect, different people might disagree with different things, and in any case Cazaux always commands consideration of his views. For example, he regards Henry of Navarre as truly religious and attached to his Protestantism, and he produces much evidence to support his contention.

Whatever the faults of the study may be, I could not help but compare it again and again with another popular work, which devotes itself to the years immediately preceding Henry IV’s accession—Garrett Mattingly’s *Armada*. The comparison is not always valid, but the fact that it is difficult to avoid constitutes the highest praise I can bestow upon Cazaux’s work.

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ALEXANDER SEDGWICK. *Jansenism in Seventeenth-Century France: Voices from the Wilderness*. Charlottesville: University Press of Virginia. 1977. Pp. xvii, 243. \$13.95.

Critics of religious movements, responding to their prejudices or interests, have interpreted Jansenism in sharply divergent ways. Whereas *philosophes* deplored its misanthropy and churchmen condemned its potential for schism, revolutionaries admired its rebelliousness and romantics praised its struggle against libertinism. More modern writers have avoided strong judgments: Marxists interpret the movement as a manifestation of the decline of the “*noblesse de robe*,” and textual analysts dwell upon tracing the origins and development of Jansenist ideas. Alexander Sedgwick’s book is in this last tradition. It is a highly polished, lucid historical portrait of Jansenism based upon a careful review of the major texts of Jansenist theologians, philosophers, and polemicists. Since there is no recent work in English that synthesizes modern scholarship in Jansenism, Sedgwick’s book meets a definite need of students of early modern France. They are likely to be misled, however, if they rely exclusively upon its conclusions.

Sedgwick argues that the theological and philosophical tenets of Jansenism set it on an inevitable collision course with religious and political authority. Chief among those tenets was the notion that an individual, when in conflict with official dogma, must rely upon an enlightened conscience in the quest for truth. More than a belief in the famous

"five propositions" of the *Augustinus* (about which some Jansenists disagreed), more than a belief in the doctrine of contrition (which other Catholics shared), and possibly more than the Jansenists' penchant for independence, solitude, and detachment, Jansenist individualism, Sedgwick argues, alienated its proponents from both the society imposed by Louis XIV and the traditional scholasticism to which Rome still adhered. If one accepts this interpretive framework, one must conclude that the Jansenists were heroes of resistance against an oppressive Church and state. But Jansenist heroism is a tarnished heroism. Victims of intolerance, they themselves were not tolerationists. Indeed, they participated vigorously in the state's crusade against the Protestants with whom, if one accepts Sedgwick's view of the Jansenists' overwhelming reliance upon an enlightened conscience, the Jansenists shared many critical values. Sedgwick glosses over this unattractive aspect of Jansenist history, saying merely that Arnauld and Nicole wrote books against the Protestants in order to "ingratiate" themselves with the crown. He then drops the matter entirely.

Sedgwick is at his best in his careful, clear explanations of the basic ideas contained in the Jansenist "assault on traditional Scholasticism," but weak in his brief attempts to relate Jansenism to French history at large. Social and political themes are too often announced and never developed. As an introduction to Jansenist theology and philosophy, Sedgwick's book is a praiseworthy, but unfortunately limited, contribution to the field.

ELISABETH ISRAELS PERRY
Bloomington, Indiana

MONTESQUIEU. *The Spirit of Laws*, with a translation of *An Essay on Causes Affecting Minds and Characters*. Edited by DAVID WALLACE CARRITHERS. Berkeley and Los Angeles: University of California Press. 1977. Pp. xxxv, 479. Cloth \$24.50; paper \$6.95.

As Thomas Pangle observed a few years ago, "The necessity and importance of the study of Montesquieu is more evident today than it has been for many generations." Montesquieu studies have flourished for some time, and David Wallace Carrithers' work is a noteworthy addition to such investigations. Further, Carrithers makes Montesquieu's own ideas and those of his interpreters readily accessible to the undergraduate and the layman.

Carrithers has been painstaking in his textual scholarship. He presents Thomas Nugent's 1750 translation of the original 1748 edition of *The Spirit of the Laws*; he has pruned the work for the modern reader, usually with care and understanding.

Footnotes indicate where this version varies from Montesquieu's manuscript and from the 1757 edition, which contained the changes Montesquieu made in response to his ecclesiastical critics. No other English edition displays such care in establishing the text. Carrithers also offers his own translation of *An Essay on Causes Affecting Minds and Characters*. Montesquieu worked on this essay from 1736 to 1743, and the undertaking was crucial in shaping his ideas on the functional relations among the physical and societal factors which form the "general spirit" or national character of a people. Carrithers has performed a great service in making this opusculum available to the English-speaking public for the first time.

The introduction is generally a helpful digest of Montesquieu scholarship. Carrithers lucidly presents biographical information and discusses the divergent interpretations of Montesquieu as a protopositivist or a disciple of the school of natural law, a liberal or a conservative, a believer in the strict separation of powers or an advocate of the interaction of the elements of a mixed government. Carrithers usually reaches well-balanced conclusions. His greatest shortcoming is his tendency to slight Montesquieu's historical writings in an effort to stress the synchronic and sociological aspects of Montesquieu's thought. Carrithers has, unfortunately, deleted the books on the history of French law from this edition of *The Spirit of the Laws*. On the other hand, he shows considerable originality and erudition in relating Montesquieu's ideas on the psychological influence of climate to eighteenth-century physiological theories, notably those contained in the works of Boerhaave and Haller.

This book is the result of meticulous research and careful reflection. It will be especially valuable to the introductory student, though the specialist certainly can profit from it. Robert Shackleton, author of the definitive biography of Montesquieu, was one of the readers of the manuscript, and Carrithers' work fully merits his magisterial imprimatur. Carrithers is now engaged in translating Montesquieu's notebooks. One can only hope that this project will meet the standards of the present book.

DAVID YOUNG
Warrensburg, Missouri

R. JOHN SINGH. *French Diplomacy in the Caribbean and the American Revolution*. Hicksville, N.Y.: Exposition Press. 1977. Pp. xi, 235. \$10.00.

Either the title of this book is misleading or the text is inadequate. Its subject is, in fact, French policies—military and colonial, as well as diplo-

matic—in the Caribbean from 1763 to 1778. The American Revolution plays a largely peripheral role throughout most of the book, except as a possible contingency affecting these policies; and the war, while occupying a significant place in the last forty-six pages, is treated only up to the Franco-American alliance of February 1778. Any reader who expects a study integrating French policy in the Caribbean with that in North America during the course of the American Revolution will, therefore, be disappointed.

Assuming, then, that the second half of the title is partly a bicentennial courtesy, what can be said of the text? R. John Singh makes a good case for the importance of the Caribbean in the overall strategy for a French war of revenge against England. Drawing extensively from correspondence in the French foreign affairs and colonial archives for the ministries of Choiseul and Vergennes, he emphasizes the role of this region as a base for supplies and troops in their plans.

The author's most original contribution is his discussion of French attempts to develop their colony of Guyane, or French Guiana (chaps. 6–7). Although the projected new colony at Kourou failed miserably, the "ancient settlement" at Cayenne was substantially reinforced in the 1760s and 1770s. Unfortunately, Singh does not indicate the specific contributions this colony made to the general Caribbean policy of France. Although somewhat less original, his emphasis on Franco-Spanish relations correctly highlights the critical position of Spain in French diplomatic preparations for war.

The most disappointing section of the book is that covering the period 1775–78 (chaps. 10–12). The only new information here involves the role of Saint-Pierre on Martinique and the activities of William Bingham, the American agent there. Aside from reference to Stephenson's 1925 article on French supplies of gunpowder to the Americans, there are no indications—even approximations—of the *amount* of French aid to the Americans. Finally, the brief chapter on the role of the Dutch Caribbean contributes nothing except puzzlement as to why it was included, particularly since any discussion of the Spanish Caribbean is expressly excluded.

Editorial bungling seriously flaws the study. Examples would take up more space than this review. Some random samples, however, include "*canonnieres*" for "*canonniers*" (e.g., pp. 18, 70, 111); two maps of the abortive colony at Kourou, but none of the Caribbean or even Guyane; the claim that in an army of 170,000 men, there were 110,000 colonels and 12,000 generals (p. 50); and excessive repetition throughout the text. The author, of course, must bear the blame for complete neglect

of Jonathan Dull's recent work. A rush to publish may have undermined a good study.

SAMUEL F. SCOTT
Wayne State University

EDWARD K. KAPLAN: *Michelet's Poetic Vision: A Romantic Philosophy of Nature, Man, and Woman*. Amherst: University of Massachusetts Press. 1977. Pp. xxvi, 211. \$12.50.

Edward K. Kaplan's monograph on Michelet's four nature works, *The Bird* (1856), *The Insect* (1857), *The Sea* (1861), and *The Mountain* (1868), follows closely upon the publication of Linda Orr's work on roughly the same subject, *Jules Michelet: Nature, Language and History* (1976). Orr's book is more perceptive and better written than Kaplan's; she is also more successful in linking Michelet's naturalism to his history and personality than Kaplan, who accepts Michelet's work and outlook on his own terms, with no attempt at psychohistory. Nonetheless, *Michelet's Poetic Vision* is clearer, if not as brilliant an interpretation as Orr's.

Michelet's romantic fascination with "animal evolution . . . underlies (and explains) his philosophy of history" (p. xviii). Evolution was not only a substitute for the Church's account of creation for this historian, who after choosing to be baptized a Catholic at age seventeen in 1816, became adamantly anti-Christian and anticlerical between 1843 and 1847 for a variety of reasons Kaplan does not fully explore. Evolution was also the levelling of the "great chain of being"—a levelling which Michelet considered a greater revolution than the democratic one of which he was the famed historian. Kaplan traces the scientific influences on Michelet's evolutionism more thoroughly than does Orr. Both these professors of French agree that much of Michelet's naturalism was "biological nonsense" (Orr, p. xiii) or "shaky nineteenth-century science" (Kaplan, p. 141), hence the sensible title "poetic vision." Specialists will still want to consult Robert van der Elst's *Michelet naturaliste* (1914), a more rigorous if unsympathetic and dated critique. Kaplan stresses Michelet's view of the harmony of nature contra Darwinism, where Orr singles out Michelet's monsters, spiders, and aggressive, predatory creatures. Kaplan convincingly shows Michelet's story of nature to be essentially the same as his story of humanity; evolution consisted in spirit's progressive emancipation from matter (which unwittingly reintroduced a hierarchy of being which Kaplan overlooks), as the peasant's biography is a long series of *jacqueries* culminating in the French Revolutionary liberation from feudalism.

Kaplan's treatment is generally well informed on the philosophical and literary side, but his stereotypes of the Church and the Second Empire are out of touch with recent historiography. His prose is empathetic, but repetitious and marred by occasional non sequiturs. It is regrettable that Kaplan did not himself translate all his quotations from Michelet, especially since his translation of the 1869 "Preface" to the *History of France* (included in the appendix) is far superior to the stilted Victorian ones used in the text.

EMMET KENNEDY

George Washington University

PHYLLIS COHEN ALBERT. *The Modernization of French Jewry: Consistory and Community in the Nineteenth Century*. Waltham, Mass.: Brandeis University Press; distributed by University Press of New England, Hanover, N.H. 1977. Pp. xxii, 450. \$27.50.

Here is a book which on several levels lacks proportion. Brandeis University Press has published it in a size and format more appropriate to *The Decline and Fall of the Roman Empire* than to an intricate, monographic study. Further, despite its portentous title, the book does not assess "modernization" with care, but rather tries to carry the day by saturation bombardment with administrative detail. Worse, the relentless assault by information obscures the historian's basic question: what is the significance of it all?

First, to publication faults. The book is strewn with stylistic inconsistencies, especially in the use of French. Some material is translated, some is not; spelling, capitalization, use of italics, and English verb tenses are all, at times, unreliable. Statistical tables often cry out for careful analysis, and sometimes jump erratically over uneven periods. One treating the frequency of central consistory meetings, for example, begins inexplicably in 1839 and then lists numbers of meetings for 1840, 1842, 1848, 1849, 1857, 1858, 1860, and 1861 (p. 97). Why are these years "samples"? What was the significance of the jump from ten meetings in 1839 to six in 1861? What was the point of the exercise? No one knows. In another case (p. 30) a table of occupations lists Jews under such uselessly crude categories as "trades," "production," "capitalists," and so on.

Modernization, in Phyllis Cohen Albert's scheme, involves two characteristics: "The first is a conscious re-evaluation of traditional values and behaviour coupled with a decision to effect deliberate (as opposed to evolutionary) change. The second is a movement toward secularization" (p. vii). Her strategy is to examine "modernization" at work in the consistorial system, the hierarchical

organization of Jewish communal life established by Napoleon in 1806. The trouble is that the very extensive consistorial, national, and departmental archives which she has so industriously consulted do not in themselves address "modernization"; at best, the texts provide building blocks for the historian. Unfortunately, Albert confuses blocks with the finished building. Thus we get long discussions of organizational structure, administrative competence, electoral routine, interorganizational rivalry—meat and drink for the bureaucrats whose writing fills the archives, but near starvation rations for readers concerned with "traditional values and behaviour" in a sustained process of re-evaluation. Albert's sometimes laborious prose betrays the priority she puts on following the file, rather than an exposition or argument: "A central consistory circular of June 12, 1848 informed the departmental consistories of the ministry's ruling [for democratic consistorial elections] and invited the consistories to reexamine the 1844 *ordonnance* in the light of the imposition of universal suffrage and the new political situation to see what changes appeared necessary" (pp. 78–79). Generalizations eventually come, but often at the end of a long march which will leave most readers exhausted, or will have seen them give up somewhere along the trail.

And yet Albert's story is not uninteresting: she outlines, in admittedly excessive length, the process by which Jewish institutions centralized, became more hierarchical, more dominated by wealthy laymen. It is clear, from her account of the consistories, how this served the interests of a liberal, reform-minded oligarchy bent on outflanking the Orthodox and bringing Judaism into harmony with the political and social culture of mid-nineteenth-century France. But it is less certain how far these developments derived from consistorial design (at one point she observes "that the central consistory generally had to be prodded by the departmental consistories or by individuals before it acted" [p. 306]) or to what extent French Jews cared about consistories in the first place. One suspects that these are the questions she dared not ask. For if the consistories had relatively little to do with "modernization," or whatever else preoccupied French Jews in this period, why write a book of four hundred fifty pages about them?

MICHAEL R. MARRUS

University of Toronto

MARVIN L. BROWN, JR. *Louis Veuillot: French Ultramontane Catholic Journalist and Layman, 1813–1883*. Durham, N.C.: Moore Publishing. Pp. 497. \$15.00.

Louis Veuillot, the epitome of militant ultramontanism, reactionary Catholicism, and aggressive

journalism in the postrevolutionary era, is the subject of an exhaustive examination by Marvin L. Brown, Jr. The major result of Brown's scholarly investigations is a comprehensive study, the first in English, of this highly controversial lay Catholic leader and his unique contribution to Catholicism in the time of Gregory XVI and Pius IX.

Identifying the three most important events in Veuillot's life as his plunge into journalism (1831), conversion to Catholicism (1838), and assumption of the editorship of *L'Univers* (1842), Brown organizes his book accordingly. Most of Veuillot's writings, notably *Les Pèlerinages de Suisse*, are analyzed with great sympathy. Similarly treated are the guidance he provided the "Catholics-before-all"; his conflicts with Montalembert, Dupanloup, Napoleon III, Gallicans, and legitimists; and the unrelenting devotion he rendered the papacy.

Perhaps Brown is too favorably inclined toward Veuillot. If the latter were "a man of the people and ever close to them" (p. 3), why was social reform so consistently abhorrent to him? With the compassion of a Cardinal Richelieu he once wrote: "Some men must work hard and live badly. Misery is a law of God to which they must submit. Society needs slaves."

Veuillot's hothouse zealotry, moreover, is treated rather casually. His sickening opinion that Luther should have been burned, and Hus burned earlier is scarcely deplored (pp. 41-42).

Neither Veuillot's association with the primitive emotionalism of the Assumptionist Fathers nor the degree to which his rabid antirepublicanism undermined French civil peace receives serious consideration. Then there is the paradox of his Catholicism and his personal behavior. As a man of presumption, gluttony, and violence, Veuillot stood condemned by the same Thomism with which he frenetically bludgeoned his adversaries.

Brown's study is further weakened by the prevalence of erroneous statements. He implies that the defeat of the Sonderbund sacrificed religious liberty in Switzerland (p. 99), confounds the Holy Alliance with the Quadruple Alliance (p. 124), seriously oversimplifies the demise of the Second Republic (pp. 162 ff.), and asserts that Veuillot sought only the conversion of the Jews and all other unbelievers (p. 249). Students of Austrian history, incidentally, will be interested to learn that Metternich detested Catholicism and royalism (p. 142).

Brown has made a heroic and commendable effort to rehabilitate Louis Veuillot. Unfortunately, such a task is virtually impossible to perform. He suggests that the most apt title for Veuillot is "sergeant of Jesus Christ." Perhaps the interests of the Church Universal would better have been served were this Christian soldier of

dubious distinction demoted to the rank of "private."

GEORGE D. BALSAMA
Kent State University

JACQUES BARIÉTY. *Les relations franco-allemandes après la première guerre mondiale*. Preface by JACQUES DROZ. (Publications de la Sorbonne. Série internationale, number 8.) Paris: Éditions Pedone. 1977. Pp. xix, 797.

The reconstruction of the history of Europe in the 1920s goes on at an exciting pace. That decade, so long a stepchild of the 1930s, has at last begun to emerge from the stereotypes and the shadows. Now the crucial subject of French power over Germany after the First World War—and the implications for Belgium and Great Britain—has been clarified in a rich and very impressive study by Jacques Bariéty.

Bariéty has literally lived with his subject for over twenty years as one of the editors of the German Foreign Ministry documents and as director of the *Institut Français* in Frankfurt-am-Main. It is one of the beauties of this book that it exhibits no anti-German prejudice; the author shifts effortlessly from the intricacies of Poincaré's intentions to Stresemann's endless calculations. Bariéty covers Franco-German relations from 1918 to 1924, with greater emphasis on the last two years; over two-thirds of this long book are devoted to the Franco-Belgian occupation of the Ruhr and the Conference of London, July-August 1924. The author has done intensive research in diplomatic and political archives, notably the German Foreign Ministry archives, the documentary collections of the *Bundesarchiv* in Koblenz, and the Millerrand and Herriot papers in the Quai d'Orsay.

This book is crammed with new material and suggestive interpretations. Here are only a few of the most important: the shifting, elusive, often contradictory Rhineland policies which the French considered and executed; their inability to choose which aspect of power in the Rhineland was most desirable—economic, strategic, or diplomatic; the impact of the Ruhr occupation on their Rhineland policies; "responsible" German separatism in the Rhineland—Konrad Adenauer in Cologne and Johannes Hoffmann in the Palatinate; Herriot's "plan" for his negotiations with MacDonald and its almost complete failure; and the adroit pressure of English and American financiers at the London Conference. Anyone who wants at last to understand what Poincaré had in his mind when he decided to occupy the Ruhr Valley in January 1923, what he said when Stresemann begged to have negotiations in September,

his indecision at the moment of victory, and how he dealt with Germans of all sorts in the crucial months of October and November can now find out.

Bariéty's fundamental thesis is that the French blew their Rhineland project at two decisive points. The first was in October and November 1923 when Poincaré, at the height of his power, let himself be seduced by the fantasy of "big" separatism in the Rhineland, separation from Germany. He gave orders—Bariéty has found the proof on this long uncertain point—to back the "advanced" separatists in the Rhineland and the Palatinate and refused to work with the "responsible" separatists, notably Adenauer and Hoffmann. "*A poursuivre une solution maximale que l'on n'atteindra pas, on laisse échapper la négociation d'une solution moyenne . . .*" (p. 261). The second point at which French policy foundered was in the summer of 1924 after Édouard Herriot had succeeded Poincaré. By his loose and self-indulgent negotiating tactics, Herriot let Ramsay MacDonald shape the decisions at Chequers and at the London Conference to his own satisfaction and that of the Germans and the English and American bankers. "*S'il est vrai . . . que Poincaré est entré dans la Ruhr en janvier 1923 non seulement pour contraindre une Allemagne rétive à céder, à payer et à désarmer, mais aussi . . . pour contrer la toute-puissance de décision anglaise dans la vie internationale, et pour acquérir un atout décisif en vue d'une négociation d'ensemble devant compléter ce qu'avait d'inachevé la construction de 1919 dans les domaines des finances internationales et de la sécurité, Herriot a laissé échapper cet atout entre le 16 juillet et le 2 août 1924, sans que la négociation d'ensemble . . . soit entamée*" (p. 598).

WILLIAM E. SCOTT
Duke University

JEAN-NOËL JEANNENEY. *Leçon d'histoire pour une gauche au pouvoir: La faillite du Cartel, 1924-1926*. Paris: Éditions du Seuil. 1977. Pp. 150.

When Frenchmen ponder the return of the Left to national power, Clio is usually asked to recall the days of the Popular Front with its memories good and bad. Jean-Noël Jeanneney suggests that an earlier leftist experience in coalition politics, the *Cartel des Gauches*, has something to teach today's aspirants for office. At first the *Cartel* model seems strangely inappropriate. The *Cartel*'s key political group, the moderate Radical-Socialist party, is a shadow of its former self, and the Communist Party, then a child of four years, was too small and too precocious to be included. The Socialists alone have strong family ties to the *Cartel* and, frankly, in the light of the present situation in France, they are embarrassing: the Socialists were reluctant co-

alition partners and, after the election, refused to join the cabinet.

Still, in the course of a good essay, the author underscores some interesting parallels. In 1924 as in 1978 the president of the Republic was hostile to the parties of the Left and actively worked against them. Millerand's resignation in the face of the opposition's victory at the polls might comfort those who fear the obstructionism of Giscard, should history repeat itself. The *mur d'argent*, as much a part of leftist jargon today as yesterday, is said to be the stuff of bad dreams, nothing more. Yet the fear of the solid opposition of the monied elite paralyzed *Cartel* leaders who sought neither to pressure nor to persuade the captains of business, banking, and industry. This was a mistake, Jeanneney argues, for even the regents of the austere and forbidding Bank of France were subject to government influence long before the bank's nationalization in 1936, because of the intense competition of interests and rivalry of personalities. The course for a modern leftist coalition to take is clear.

Apart from specific then-and-now comparisons, Jeanneney's plea to politicians of the present is for honesty, candor, hard work, and action. In the 1924 scenario Édouard Herriot, the Radical-Socialist premier, is the villain *malgré lui*. The portly, loveable mayor of Lyons was well educated, sympathetic, and an orator of surprising passion. Yet it is hard to disagree with Jeanneney on his shortcomings. "Others" always seemed to be responsible for his delays and difficulties: the Socialists for not cooperating wholeheartedly, the Right for sabotaging his financial plans, the British and Germans for taking advantage of his instinctive generosity, the Americans for posing impossible conditions for money borrowed or to be borrowed, and his own finance minister for refusing to stand by his tax schemes. In addition, at crucial moments he lacked stamina, expertise, and judgment. Faced with the homework required for diplomatic talks, he wilted. Confronted with matters economic, which he didn't understand at all, he sadly held his head and moaned. Told that previous governments had masked the true state of French finances by falsifying the published reports of the Bank of France, he went along with the sorry charade. Perhaps most serious of all, Jeanneney believes that largely because of Herriot's mistakes, the *Cartel* was handed over bound and gagged to the international money men in New York.

While Herriot deserves credit for some of the *Cartel*'s misfortunes, many things were out of his hands. France's "shameful" dependence on American financiers is one example. Even Poincaré, a far stronger man than Herriot, realized that the French crisis required more than emergency tax

measures and internal reorganization. American capital had become vital to France's financial reconstruction and essential to unraveling Europe's reparations tangle. Jeanneney echoes Herriot's indignant wail—on hearing the American terms for loans—that France, after all, was not Turkey. Not everyone would have agreed. Contemporary experts, many of them French, feared that France was fast becoming the sick man of Western Europe. Unfortunately, American doctors (of the financial sort) have never had the most gentle bedside manner.

In sum, Jeanneney provides both a history lesson and a primer on political ethics. Neither is completely satisfying, but he jars the memory of those who might prefer to forget and recalls the responsibilities of those who seek to rule.

WILLIAM A. HOISINGTON, JR.
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DAVID H. WEINBERG. *A Community on Trial: The Jews of Paris in the 1930s*. 1st English edition. Chicago: University of Chicago Press. 1977. Pp. xvi, 239. \$18.50.

The tragedy of the European Jewish communities on the eve of World War II was that no amount of political acumen, good will, or rational action could have enabled them to defend themselves against the Nazi onslaught. Parisian Jews, who comprised the second largest Jewish community of Europe (after Warsaw), were in no better position than others to protect themselves. Inevitably, however, they have been subject to the same harsh scrutiny and criticism that has been the post-Holocaust fate of the rest of European Jewry. When David Weinberg's study of Parisian Jewry in the 1930s was first published in France in 1974 it was eagerly studied for the light it might shed on the Jewish response to the rise of Nazi Germany. Although it might seem that his description of the fragmented Paris Jewish community lends support to the charge that the Jews were somehow guilty of complicity in their own destruction, Weinberg insists that "a historian viewing the events with forty years' hindsight . . . must conclude that there was little that Jews could have done directly before and during the Holocaust to physically prevent their annihilation" (p. x). He blames Jewish leadership only for the failure "to create a common awareness of the Jewish plight" (p. xi). Although unity could not have saved their lives, Weinberg argues, it would have "performed a vitally important psychological function by providing the basis for a commitment to maintain Jewish solidarity against the Nazi enemy" (p. xi).

A Community on Trial is an in-depth study of the organized Paris Jewish community of the 1930s. It examines demographic, social, cultural, and political aspects of French Jewish behavior. It describes the two very different Jewish populations: the wealthier, acculturated, native French Jews, and the recently arrived Eastern European Jews. The immigrants' legal status was doubtful, their socioeconomic status was depressed, and their geographical location and cultural, political, and religious orientations hardly ever overlapped with those of the natives. Weinberg describes a myriad of separate religious, political, social, and ideological organizations of native Jews and of immigrant Jews. Prospects for unity thus appear to have been doomed from the start.

Unfortunately, in introducing his otherwise excellent analysis of the competing notions of Jewish identity in the 1930s, Weinberg distorts the historical dimension of this problem. He states (p. 45) that "the question 'What is a Jew?' has perplexed Jews in all ages." In fact, the question is a recent one, arising only in the eighteenth century, when new versions of what constitutes Jewish identity began to compete with the traditional one of corporate Jewish religio-national identity.

Weinberg's study is based on French- and Yiddish-language periodicals, the archives of several Jewish organizations—the consistory, the Bund, the *Alliance Israélite*—and personal interviews and correspondence with people active in the community during the 1930s. It is painstakingly researched, well written, and beautifully documented. The many long notes make almost as interesting reading as the text itself. The book is a rare example of excellence in Jewish community studies.

PHYLLIS COHEN ALBERT
Harvard University

WILFRIED LOTH. *Sozialismus und Internationalismus: Die Französischen Sozialisten und die Nachkriegsordnung Europas, 1940–1950*. (Studien zur Zeitgeschichte.) Stuttgart: Deutsche Verlags-Anstalt. 1977. Pp. 414.

Sozialismus und Internationalismus tells us how French socialist aspirations for a peaceful and stable Europe evolved during a decade stretching from the fall of France to the Schuman Plan. It is based on the unpublished and published minutes of party conferences, as well as an eighteen-page bibliography of printed texts and secondary studies. Wilfried Loth divides the period under review into a large number of tiny chronological segments and devotes a chapter to each. Thus he traces in carefully documented detail how the SFIO moved from advocacy of world government to support of West

European integration, from socialist solidarity to anti-Communist sectarianism, and from the role of spokesman of rising socialist expectations to that of spectator of the first steps into the supranational unknown.

Following the author on his slow and meticulous progress through the verbal and ritual thickets of French political controversy, we learn little that is new or exciting. Since the bulk of Loth's documentation consists of printed sources readily available at the time of the events covered, it is difficult to see how it could be otherwise. The book, therefore, mainly reconfirms what we have long known: after the war French socialists hoped for peace and counted on superpower harmony. They expected to enter a productive and profitable partnership with the Communists and advocated Germany's early return to the community of nations. The evidence likewise reconfirms that the violent death of Jan Masaryk constituted the single most jolting event in the evolution of the SFIO's postwar foreign policy attitudes.

The author attempts to dispel the aura of *déjà vu* with the promise that his findings will answer six questions concerning "theory and practice of socialist foreign policy in France" in terms of goals, methods, attitudes, and efforts. But his conclusions are curiously ambivalent and do not really constitute unequivocal answers. On the one hand Loth credits Léon Blum's influence for almost everything positive achieved by the SFIO in foreign affairs. But at the same time he insists that his case study demonstrates that socialist intentions cannot be realized by a purely national policy effort. One may charitably argue that the author has succeeded in finding one answer to all six questions, or else suspect that he had forgotten the questions by the time he came to the end of his manuscript.

Nevertheless, Loth's long, slow, and detailed chronicle of socialist foreign policy, heavily loaded with quotations, remains readable because he has put it together with skill and clothed it in fastidious prose. One comes away feeling that his talents should have been lavished on a more exciting and important topic.

HANS A. SCHMITT
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LOIS PATTISON DE MÉNIL. *Who Speaks for Europe? The Vision of Charles De Gaulle*. London: Weidenfeld and Nicolson. Pp. viii, 232. £7.50.

There is a warm nostalgia among gallophiles for what now seem to be the heroic politics of the 1960s and for Charles de Gaulle's alluring and mon-

umental diplomatic style. Lois Pattison de Ménil's study of de Gaulle's European vision and of the obstacles to its attainment imposed by the United States and by the Federal Republic of Germany is a graceful, sympathetic, and frequently eloquent account of the central issues in the politics of Europe and the North Atlantic alliance during the last decade. Liberally using de Gaulle's own words, de Ménil succinctly portrays the objectives of Gaullian foreign policy and the interplay between government and society in France, Germany, and the United States during the critical period of the so-called construction of Europe. While the study demonstrates the author's great empathy for French politics, it also fairly treats the aims of de Gaulle's principal detractors and the political assumptions that propelled them.

If the central strength of this study resides in its elegant formulations and synthetic overview of de Gaulle's diplomacy, its major contribution is its analysis of Franco-German relations. The author properly puts France's bilateral ties to Germany at center stage in de Gaulle's foreign policy in the 1960s. Setting the scene with sweeping but digestible statements to place these bilateral relations in their historical context, de Ménil then traces the fortunes and dilemmas of de Gaulle's European politics to the vagaries of domestic politics in Germany. As a scholar, the author piquantly focuses upon the central personalities and issues and uncannily captures in her carefully selected anecdotes the main scenes in the ballet of Franco-German-American relations. As a writer, she has provided a crisp and alluring tale which, in the hindsight of the decade that has now passed since de Gaulle's resignation from office, has lost little of its original fascination.

The study's weaknesses are found in the sections dealing with economic diplomacy, which, unlike the more important chapters dealing with Franco-German politics, are given short shrift. De Ménil's critique of Kennedy's grand design is also uncharacteristically overstated. She shows little sympathy, for example, for the strategic motivations behind the American position toward France's nuclear force and her criticism of the Multi-Lateral Force (MLF) in NATO is contradictory—the MLF did enjoy support in Europe, including Germany.

These weaknesses do not, however, detract unduly from the study. It is the most concise, balanced, and fair treatment of de Gaulle that this reviewer knows. Both the graceful style and the original contributions on Franco-German relations should make this study an enduring scholarly effort.

EDWARD L. MORSE
Council on Foreign Relations

PAUL CLAY SORUM. *Intellectuals and Decolonization in France*. Chapel Hill: University of North Carolina Press. 1977. Pp. xiv, 305. \$16.95.

Why did French intellectuals have such a hard time swallowing the idea of colonial independence? Because, Paul Sorum replies, independence did not square with "their most fundamental values and beliefs." Sorum chose ten intellectuals to represent the views of thousands who might lay claim to the title. He read the writings of the ten on the colonial question, interviewed some of them, and took soundings in the press of the Left. The intellectuals, he contends, could not shake off the conviction that freeing the colonies would be abandoning newborn lambs to a world of hungry wolves. A belief in France's civilizing mission, an awareness of the pitfalls of economic development, an urge to right the wrongs of the past, a fear of the old possessions falling victim to a new imperialism, all supported the view that French tutelage was preferable to freedom. As France yielded territory, the intellectuals yielded the old convictions. Finally, the Algerian crisis brought them to accept as legitimate the demand for independence.

For all the influence they exerted on the conduct of public affairs, Sorum's ten intellectuals may as well have been ten pastry cooks. If they awakened politicians and public to the issues of colonialism, they did not succeed, once they had embraced the idea themselves, in persuading any policy-makers of the need for independence.

Jean-Paul Sartre and Raymond Aron make odd members of Sorum's reluctant ten. Both, for vastly different reasons, were enthusiasts of independence. Jean-Jacques Servan-Schreiber is another odd choice. Servan-Schreiber's reluctance to embrace colonial independence qualifies him for membership, but whether this journalist-entrepreneur should be described as an intellectual is debatable. "Intellectuals," like "the middle classes," continue to confound the historian's efforts at classification.

Early and late chapters take up such matters as the French Union and ethnocentrism. Looking at decolonization through such lenses as these is like watching a street fight through the wrong end of a telescope; but the chapters on Algeria, filled with eye-gouging polemic, put the reader at curbside.

The Algerian war continues to provoke sharp disagreement. Keeping company with the intellectuals has understandably caused Sorum to take on many of their views. Many of these views are open to question and to doubt. Integration was less "an updated version of assimilation" than it was a dubious scheme for keeping the settlers French and allowing the Algerians to remain Algerian. To blame French vigilantes for the round of

terrorism that set off the Battle of Algiers is too readily to accept Germaine Tillion's view of that episode. The war produced a lot of talk about "fascism," but very few fascists. Sorum makes too much of this bugaboo of the intellectuals and smear-word of the Communist Party.

Sorum's study of ten who did not rule might have benefited from more mention of a few who did. Too much of the action that brought the end of empire takes place offstage.

JOHN TALBOTT
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JOSÉ RUBIA BARCIA and SELMA MARGARETTEN, editors. *Américo Castro and the Meaning of Spanish Civilization*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xix, 336. \$14.50.

AMÉRICO CASTRO. *An Idea of History: Selected Essays*. Translated and edited by STEPHEN GILMAN and EDMUND L. KING. Introduction by ROY HARVEY PEARCE. Columbus: Ohio State University Press. 1977. Pp. x, 343. \$12.50.

An Idea of History comprises a collection of essays in literary criticism, literary history, and historiography (previously published partly in Spanish and partly in English) on which Castro's reputation as a distinguished Hispanist has been established. Some of the essays included are original translations from the Spanish and therefore provide a new selection in English of Castro's published and unpublished writings. The essays reveal the diverse and broad range of Castro's scholarly writings and present his thoughts on the art of Cervantes, the picaresque novel, the Spanish classical theater, the teaching of Spanish, philological problems, and history. They also reflect his continuous concern with assessing the meaning of Spanish civilization as a single cultural, social, and historical unit and with analyzing the roles of the intimate, and often intricate, interrelationship of Christianity, Islam, and Judaism in the history of Spain.

In direct disagreement with the "Europeanizing" view of Spain, Castro contended that the present character of the Iberian peninsula must be viewed as a special historical phenomenon to which the historical categories of other European nations are not applicable. He attempted to understand the "history of the Spaniards" as a peculiar historical phenomenon in which "Spanish Christians, Moors, and Jews" lived together as a collective cultural unit. The specific moment in the past when these three distinct "castes" of believers began this "living-togetherness" (*convivencia*) occurred after the coming of the Muslims to the

peninsula in 711 and initiated a way of life unique to Spain. Islamic emphasis on religious tolerance permitted the harmonious coexistence of "Hispano-Christians, Hispano-Jews, and Hispano-Muslims" until the end of the fifteenth century. The emergence and eventual dominance of the Christian caste—which Castro identified as the groups of Leon, Castile, Aragon, and Catalonia—led to the exclusion, subjugation, and expulsion of the other two castes beginning in 1492. After the expulsion of the Jews and Moors from Spain, which broke apart pre-existing harmony, respect for the "pure lineage of people" became manifest and the "honor" of being a "Spaniard" began to occupy the whole range of self-identity. The dominance of the Christian caste, from the seventeenth century to the present, contributed to the inevitable disintegration of the three castes and intensified awareness of the value of the Christian caste as being non-Jewish or non-Muslim. "It is to this harmony and tearing apart of castes," Castro maintained, "that the slice of human reality known as the Spanish people owes its originality, its great achievements, and its problems" (p. 188).

Castro's ideas about medieval Spain and the role played by Moors and Jews in its formation were the most important insights made possible by his particular approach to Spanish historiography. Indeed, he argued that without the circumstances of the previous intermixture of the three castes of believers and their subsequent "purism, tension, and tearing apart" between 1492 and 1609, "neither *La Celestina* nor *Don Quijote* would exist, nor would the Empire have been structured in that form, nor would it have been economically unproductive. . . . Nor would the Spaniards have . . . fallen into the ignorance and intellectual collapse of the eighteenth century—a serious mortgage still not entirely paid off" (pp. 115–20).

Américo Castro, compiled by friends, former colleagues, and students, is a collection of fifteen essays one of which was written by Castro and six of which were presented originally as lectures in a symposium commemorating Castro at UCLA in 1973. The essays address several of the problems raised in Castro's scholarly writings, and according to the editor, were intended to be neither a *Festschrift* nor a *Mélanges*, but to offer the "English-speaking world a systematic organization of Américo Castro's thought and theories" (p. vii). The organization of the book reflects the general categorization of Castro's works: "Part I, Prologues"; "Part II, On the Formation of Spain's Vital Structure"; "Part III, on Spain's Age of Conflict"; "Part IV, On the Modern Period"; and "Part V, Epilogues." José Rubia Barcia writes about the name, Américo Castro y Quesada; Guillermo Araya Goubet analyzes the evolution of Castro's

theories; James T. Monroe discusses aspects of the Hispano-Arabic world; Emilio González-López examines the role of the myth of St. James the Apostle and its functional reality with respect to Santiago de Compostela; Julio Rodríguez-Puértolas presents a comprehensive view of medieval Spain. The remaining essays discuss a variety of topics: the Spanish Jews by Joseph H. Silverman; a different perspective of Cervantes' work by Anthony A. van Beysterveldt; the classical theater and its reflection of Spanish life by Carroll B. Johnson; the so-called "Spanishness" of the eighteenth century by Enrique Rodríguez-Cedpeda; an appraisal of the immediate past and present of Spain by Rubén Benítez; a comparative discussion of Américo Castro and José Ortega y Gasset by Franco Meregalli; the Hispanic origins of Iberoamerica by Marcos A. Morínigo; a view of a new model for Hispanic history by José Luis L. Aranguren; and an examination of literature and historical insight by Stephen Gilman.

Despite a certain degree of repetition, the contributors attempt to synthesize Castro's themes and they emphasize the importance of his scholarly works for Iberian and Ibero-American studies. As one contributor writes: ". . . at the present time it is almost impossible, in absolute terms, to approach almost any point of Spanish history and culture without stating one's position in regard to Castro's doctrine. . . . One must either yield to his doctrine or have very good reasons for not doing so" (p. 64).

OLIVER W. HOLMES
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J. A. FERNÁNDEZ-SANTAMARIA. *The State, War and Peace: Spanish Political Thought in the Renaissance, 1516–1559*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1977. Pp. xiv, 316. \$24.50

In the second volume of *The New Cambridge Modern History* G. R. Elton suggested that, "generally speaking, significant political thinking occurs only when there are significant political upheavals. Spain, as is not surprising in view of its pretty placid internal state, could show no writings of note" (pp. 458–59). This book disputes Elton's opinion. The author believes that important Spanish political writing resulted from two early sixteenth-century developments: the accession of a Habsburg ruler to the Spanish throne and the discovery and exploration of the New World. These events forced political theorists to reassess traditional views in light of Habsburg imperial policies.

Within this framework J. A. Fernández-Santa-

maria examines the work of several Spanish thinkers. Beginning with Alonso de Castrillo, an opponent of empire in the 1520s, the author shows how Spaniards came to appreciate and defend imperialism. The greatest attention is given to Francisco de Vitoria and Juan Ginés de Sepúlveda; other writers considered here include Alfonso de Valdés, Juan Luis Vives, Diego de Covarrubias, Antonio de Guevara, and Fadrique Furió Ceriol. Detailed analysis of the work of these men demonstrates that Elton's offhand dismissal of Spanish political thought is unwarranted.

Fernández-Santamaria has made several attempts to deal with his subject in a broader perspective. These instances include an excellent note (p. 33) in which he finds that J. Russell Major's definition of a "typical" Renaissance monarchy (in "The Renaissance Monarchy as seen by Erasmus, More, Seyssel, and Machiavelli") does not apply to Castile, and an entire chapter on "The Age of Erasmus on War and Peace." Because the question of the "just war" cannot easily be isolated from other aspects of sixteenth-century political thought, this chapter does not contribute significantly to the book. As the author himself notes, "What conclusions can we draw from this brief scrutiny of noted Christian humanist apologists for peace? In reality none that have not already been suggested" (p. 158). Nevertheless, this attempt to compare Spanish writers with non-Spaniards such as St. Augustine, More, Rabelais, and Menno Simons is interesting, especially in view of the assertion that "with the exception of Machiavelli's cold pragmatism all currents and themes prevalent in the European political thought of the day were well represented in Spain . . ." (p. 119).

Although each chapter is well organized, this book is not easy reading. Much of the difficulty comes from the author's graceless writing style. His sentences are often wordy and complex. Fernández-Santamaria has obviously worked hard to produce this useful book, but the reader will have to do the same to get through it.

ROGER SCHLESINGER
Washington State University

BAILEY W. DIFFIE and GEORGE D. WINIUS. *Foundations of the Portuguese Empire, 1415-1580*. (Europe and the World in the Age of Expansion, number 1.) Minneapolis: University of Minnesota Press. 1977. Pp. xxx, 533. \$25.00.

This rather awkwardly constructed volume really consists of two works clobbered together. Chapters one to thirteen and appendixes one to sixteen, dealing with the period 1415-1500, are by Bailey

W. Diffie; chapters fourteen to twenty-two, covering the years 1500-80, are by George Winius. They wrote their respective parts separately, though in consultation with each other. Their joint aim was to show that "Portuguese expansion was the result of long-trend economic and maritime developments, rather than the work of one man. Portugal led all other nations in revealing the world to Europe, but there is no basis for 'secret' discoveries before the known voyages of Dias, da Gama, Cabral, and others, and . . . Portugal's overseas administration set the model that other nations copied and modified" (p. xii).

Diffie is pushing at an open door. It is many years since any serious historian maintained that Henry the Navigator was the sole inspiration for Portuguese overseas enterprise in the fifteenth century. The leading Portuguese historians in this field—Duarte Leite, Magalhães Godinho, Teixeira da Mota, Luís de Albuquerque—never subscribed to this simplistic version. What Diffie gives us is an objective and conscientious narrative of the fifteenth-century discoveries, relying on the published documentation and on analytical secondary works by the most competent authorities on both sides of the Atlantic. He does not come up with anything new or startling, and there is no reason why he should have done so, in the light of our present knowledge. He is clearly much more interested in the minutiae of the actual discoveries—who discovered what and when—than in the structure of fifteenth-century Portuguese society, which one might think needs a more detailed analysis in a work professedly dealing with the foundations of empire. For this, the reader will do better to go to Magalhães Godinho's seminal *Estrutura da antiga sociedade portuguesa*, now in its third edition (1977) and to volume one of A. H. de Oliveira Marques, *History of Portugal* (1971-72). Diffie maintains that "the Portuguese obviously were a commerce-minded and sea-minded people" (p. 38). I am more inclined to agree with Orlando Ribeiro and Raquel Soeiro, who maintain that they were primarily land-based peasants with their backs to the sea.

George Winius' section, dealing chiefly with Portugal's Asian empire, is more innovative. He is mainly concerned "to examine the Portuguese empire as a unit and to explain what factors made it possible and motivated it, and what ones inhibited its ultimate success" (pp. xx-xxxi). He rightly stresses King Manuel's crucial role in implementing a vigorous and determined policy of expansion in Asia, despite the misgivings of most of this councillors, and despite the daunting obstacles, physical and logistical. Winius' analysis of the "ruinous reign" of King John III, 1521-57 (pp. 280-85) is soundly based and convincingly ex-

plained. He correctly emphasizes the crippling role of the continued obsession of Portuguese monarchs with the mirage of the conquest of Morocco—shared, incidentally by Luís de Camões, “poet of the Faith and of Empire.” He reminds us that “self-aggrandizement at the expense of the crown in fact proved to be a constant throughout the history of the Portuguese Asian empire” (p. 244), and was not confined to Afonso de Albuquerque’s mutinous captains at Ormuz. In a suggestive chapter entitled “The Balance Sheet,” he makes the point that even in the palmiest days of “Golden Goa,” the crown normally had to borrow the capital in Europe to finance its next year’s cargoes of pepper in India (pp. 433–34). On the other hand, his chapter on “Institutions of Trade and Government” is not so enlightening as J. B. Harrison’s articles in volumes three and four of the *New Cambridge Modern History*. Consultation of the densely documented articles by Jean Aubin, Geneviève Bouchon, and others in *Mare Luso-Indicum* (three volumes to date, 1971–76) and of relevant articles in *The Indian Economic and Social History Review* would have saved him from several errors and omissions.

The book is well produced and in paperback form should prove useful in the student market for which it is clearly intended. But if we give—as we should—an A-plus to the second volume in this series, Holden Furber’s superb *Rival Empires of Trade in the Orient, 1600–1800*, we cannot give this one more than an A-minus.

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J. A. VAN HOUTTE. *An Economic History of the Low Countries, 800–1800*. New York: St. Martin’s Press. 1977. Pp. ix, 342. \$18.95.

Unfortunately and unjustifiably this book will probably reach only a small audience. Despite the importance of the Dutch and Belgian economies as examples of success through the Middle Ages and up to the eighteenth century, the economic history of the Low Countries is unknown to most readers. The usually insurmountable barrier of language has been removed in this case by J. A. van Houtte’s own translation from Netherlandish, a translation which shows only a rare sign of clumsiness.

Van Houtte has already made extensive contributions to the study of Flemish and Brabantine economic history, especially of the fifteenth and sixteenth centuries. Recently retired as a professor of history at the Catholic University of Louvain, this renowned scholar has pieced together a survey

of the economic development of the region of the modern Benelux countries. The history begins with the collapse of the Carolingian Empire and the High Middle Ages, passes through the crisis of the fourteenth and fifteenth centuries, covers the golden age of the economy down to 1670, and then deals with structural changes and problems of the years to 1800. Van Houtte includes not only the traditional topics of Low Country economic history such as trade, industry, and the fisheries, but also those fields which have been subject to the greatest interest and success in research over recent years such as demography and agriculture. He also discusses topics often ignored, for example money and credit. While he claims that he is writing both economic and social history, the emphasis is overwhelmingly on the former. He reports the results of a broad range of recent work and does not hesitate to quote data of many different types from that work to illustrate his description.

In such a general survey there is always something missing. Surprisingly van Houtte does not devote much consideration to the question of the change of Flanders from active to passive trade in the High Middle Ages. He does not go into detail at all in mentioning the precocious industrialization of the Austrian Netherlands in the eighteenth century. The greatest drawback to the book, however, is the lack of analysis. Explanation of events described is limited, often hidden, and usually based on exogenous forces. The two-page conclusion is nothing more than a summary of the survey. The index is a good one, so it is easy to find information; however, the lack of notes makes it difficult to pursue the sources for data. The bibliography is excellent and up-to-date, if short. Above all the entire story is here, from the large estates of the ninth century, to the first drainage projects in the twelfth, to the founding of the first bourse in Bruges in the fourteenth, to the international goods and money market at Antwerp in the sixteenth, to the Baltic grain trade through Amsterdam in the seventeenth, to the relative stagnation of the Dutch economy in the eighteenth. The treatment throughout reflects the measured judgment of a scholar who has spent a lifetime studying this history.

RICHARD W. UNGER
University of British Columbia

DERIC REGIN. *Traders, Artists, Burghers: A Cultural History of Amsterdam in the 17th Century*. Assen: Van Gorcum; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. x, 214. \$19.00.

Amsterdam, especially Amsterdam in its golden age, continues to attract the interest of historians.

Deric Regin's "act of piety," as he calls it (p. ix), is the latest entry in this fairly large field of inquiry.

After introductory chapters stressing the city's dependence upon the sea and seaborne commerce, Regin examines several aspects of Amsterdam's culture during the seventeenth century: the various religious groups and their influence on the visual arts and music; literary expression, history, and biography; philosophic and scientific study (including Descartes and Locke), printing, and cartography; financial concerns centering about the stock exchange, the shipbuilding industry, and the Tulipomania of 1633-37; development of the theater; social and political affiliations of the city's painters; and, finally, renewed power of the House of Orange, increasing self-satisfaction of the oligarchy, and intrusion of French tastes and fashions coinciding with the decline in creative energy.

The foundation of Amsterdam's unique bourgeois culture was, Regin argues, a sudden consciousness of independence, a decisive confidence born of that independence, and a loyalty to their city equaling that of the ancient Greeks to their polis. The golden age of Amsterdam was a deliberate celebration of freedom.

With their pragmatic and empirical tastes already set and utility uppermost in their scale of values, the post-1578 citizens of Amsterdam evolved an intimate Baroque style of restrained, inward vibrancy, totally unlike the aristocratic-absolutist life styles of their contemporaries in England, France, and Spain. Characteristic of this intimate Baroque style was: poetry that exalted naval exploits and merchandising rather than the beauties or mysteries of the sea; warehouses whose architectural merit lay in their functionalism; churches with clean, sober, whitewashed interiors that were more houses for God's people than they were houses of God; an end to the great Netherlandish school of music but the development of carillons; didactic Emblem books of poetry stressing practical wisdom; an intense interest in local history, experimental and utilitarian science (Van Leeuwenhoek), philosophy, mapmaking, mechanics, and printing; establishment of numerous public orphanages, hospitals, workhouses, and homes for the elderly; group portraits of regents, syndics, and civic guards; hygienic and comfortable homes; still-life paintings in which flowers, fish, game, or ordinary household utensils, for example, became objects of beauty; and depictions of domestic interiors steeped in intimacy, respectability, and *gezelligheid*, etc., etc., etc. Though little of this is new, there is an advantage in having it all under one, convenient cover.

Unfortunately, Regin does not make the most of his material or thesis. Despite some interesting observations, for the most part he offers neither a

good story nor a profound analysis. His book often reads like a descriptive catalog, a catalog without index or bibliography (two very serious omissions). The text is marred by many typographical errors and the subject demands more than eight illustrations. Regin's work will have its uses, but the services of a good editor and a careful proof-reader could have made it much better.

FRANK T. BRECHKA
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Berkeley

MICHEL THEYS. *La population de Diest pendant les deux premiers tiers du XIX^e siècle*. (Collection Histoire Pro Civitate, Series in-8°, number 50.) Brussels: Crédit Communal de Belgique. 1977. Pp. 166. 300 F.

Diest is a pleasant little town in northeast Belgium that is probably best known for its charming sixteenth-century beguinage. Michel Theys has written a monograph about the population of this town from the beginning of the French regime in Belgium until the third Belgian national census of 1866. During this era Diest grew from about 5,400 inhabitants to about 7,200 inhabitants (excluding the 500 soldiers in its garrison), even though its minor clothwork and distilling industries virtually disappeared. It has not grown much since; in 1970 about 9,500 people lived there. Although Theys discusses all the requisite topics for a study of demographic history—sources; population and habitat; births, marriages, and deaths; migration; nuptiality and family size; mortality—this book is disappointing.

While Theys deals adequately with his sources, he has essentially compiled a book of tables that might serve as source materials for a more imaginative demographer. Even if we ignore appendixes, more than half the book's 136 pages are tables or figures. It is methodologically primitive, despite the fact that he makes use of the town's population register, one of the most unusual and useful sources for analysis of historical populations ever compiled. He computes no life tables nor sophisticated measures of fertility, and he ignores the potential for computing differential rates of births or deaths according to occupation or social class. Perhaps most important, he stops just before fertility began to decline in nineteenth-century Belgium, and thus ignores a crucial question.

Theys presents no thesis whatsoever. He does identify a number of interesting facts, among them that, in 1846, 38 percent of all families (668) lived in single rooms, and that 24.3 percent of all brides were pregnant. Unfortunately, he does nothing to explain these curiosities, or to develop any of the other facts he discovers and then dismisses in a single sentence. His comparisons are limited to a

single French monograph. Considering the publishing crisis that reigns in the United States today, it is something of a wonder that a reputable series of some international standing would publish a non-book such as this.

MYRON P. GUTMANN
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Austin

JOHN GILLINGHAM. *Belgian Business in the Nazi New Order*. Ghent: Jan Dhondt Foundation. 1977. Pp. 237.

Collaboration in Belgium in the Second World War has usually meant the extremist Rex groups of Léon Degrelle and the VNV Flemish nationalists. John Gillingham changes all that, for he thoughtfully delineates a high-level collaborationist policy led by the king of the Belgians and the major banking circles. Previous studies of Nazi-occupied economies have bared a similar pattern, of the vanquished cooperating with the conqueror. Gillingham's penetrating analysis of the Belgian variant finds the compelling motives of needed modernization, larger profit, more state security, and "the exercise of co-determining influence in state economic policy and the maintenance of a favorable climate of domestic politics" as powerful incentives that linked big business to the German war effort. Crudely put, the Belgian "politics of production" meant peace, material prosperity, and long-sought-after economic change under German direction, rather than endless fighting and the severity of hunger, brigandage, disease, and deportations.

The first half of this brief revised dissertation deals with the pre-1943 Belgian-initiated agreements of participation in the German war machine. The author insists that Nazi economic policy was disorganized and decentralized but successful because the Belgian banking-commercial-industrial elite was receptive to structural reforms that would combine conservative-authoritarian rule, a technocratic state administration, and reorganization of the modernized Belgian production and exchange system. These new practices and policies instituted during the war—the "organized and directed" capitalist economy—were notions of those who interpreted the interwar period in Belgium as proof of the inadequacies of parliamentary democracy and economic liberalism. As elsewhere, fascist rule acted as a precipitant to changes in business methods and outlooks in Belgium. Gillingham illuminates the similarities with Vichy France and its National Revolution, and they are indeed striking.

The larger second part of the work focuses on the development of mobilization for total war in

Berlin and for Belgium during 1942-43. These chapters will be of great interest to Nazi era historians, for they illustrate the chaotic weaknesses of the civilian-military divisions of authority for the regions seized (primarily the *Aufsichtsverwaltungen*, *Wi Rü Amt*, and *Luftwaffe*) and the emergence of the overhauled and reoriented Nazi machinery of the planner-technocrats like Todt, Kehrl, and Speer. In two telling chapters, the author asserts and demonstrates how the black market and the breakdown in coal production and transportation were prime reasons for the German failure to exact more from Belgium.

Collaboration did not mean the absence of resistance, but this case study of the interrelated Belgo-German wartime economic dynamics offers another perspective behind Belgian business behavior. It appears to have been based on the bureaucratic elites' desire to escape the prewar Malthusian "lock-step" via close cooperation with Hitler's "new economic order." Furthermore, these advocates sought the erosion of barriers between business and state groups responsible for financial and economic policy, positing that the welfare of business and state were identical. To these business reformers, the moral and ideological characteristics of the Nazis were less significant than what was done for business by the National Socialist regime.

Gillingham's thesis has been challenged (*Revue belge d'histoire contemporaine*, 1-2 [1974]), but this closely and tightly documented, no-nonsense book presents a convincing picture that could not be produced by a Belgian even today. It is difficult to argue against the evidence that, for business leaders, collaboration not only made economic sense, but resulted in moderate overall production increases in earning and capital. It also greatly augmented business power to influence public opinion, legislators, and state policy in the postwar era, when many of these reforms were fully legitimized and institutionalized.

This meaty account, probably as thorough as available sources permit, mobilizes evidence skillfully and reads easily. An obscure Belgian press and inadequate publicity will limit the book's circulation, and minor flaws and petty errors (many due to a foreign language editor and typesetter) mar a basically lucid narrative. This does not alter the fact that the book compares favorably with the Milward, Sarti, and Warmbrunn explorations, and fills the vital need of viewing the other side of the wartime coin in Belgium.

PIERRE-HENRI LAURENT
Tufts University

HEINRICH BEST and REINHARD MANN, editors. *Quantitative Methoden in der historisch-sozialwissenschaft-*

lichen Forschung. (Historisch-Sozialwissenschaftliche Forschungen, number 3.) Stuttgart: Ernst Klett-J. G. Cotta'sche Buchhandlung. 1977. Pp. 254.

WOLFGANG BICK *et al.* *Quantitative historische Forschung 1977: Eine Dokumentation der QUANTUM-Erhebung.* (Historisch-Sozialwissenschaftliche Forschungen, number 1.) Stuttgart: Ernst Klett Verlag. 1977. Pp. 236.

PAUL J. MÜLLER, editor. *Die Analyse prozess-produzierter Daten.* (Historisch-Sozialwissenschaftliche Forschungen, number 2.) Stuttgart: Ernst Klett-J. G. Cotta'sche Buchhandlung. 1977. Pp. 226.

In the last decade the paradigm change toward social history, the move toward theoretical models, and renewed interest in the social sciences have laid the groundwork for the belated adoption of quantitative methods by German historians. Since its foundation by young historians and sociologists at the University of Cologne in 1975, the organization QUANTUM (with over three hundred members) has vigorously promoted quantification through a newsletter (*QUANTUM-Information*), a lecture series, panels at the German historians' and sociologists' conventions, an international conference with the Social Science History Association, several specialized meetings, the creation of networks, and the launching of a new publication series, *Historisch-Sozialwissenschaftliche Forschungen (HSF)*.

Its first volume, *QUANTUM Dokumentation*, presents an exhaustive survey of 270 self-defined quantitative research projects in German-speaking countries (and another 35 outside), which demonstrates both the astounding growth of quantification and its continuing difficulties (no GDR projects are included). A comparison with social science research shows developmental deficits such as manual data processing (50 percent), single person research (83 percent), descriptive cross tabulation (47 percent), degree-directed work (55 percent), and a lack of previous experience in the use of statistics or program packages (55 percent). As correctly stressed in the introduction by W. Bick, P. Müller, and H. Reinke, the returns nevertheless reveal a strong minority of sophisticated computer-based projects with analytical and interdisciplinary orientations, which augurs well for the future. The printing of complete questionnaires makes *QUANTUM Dokumentation* a useful reference guide to current research in economic, social, and political history, even if the coverage of North American projects is uneven (cf. the companion survey of the quantification committee of the Conference Group for Central European History, forthcoming in *Central European History*).

The second volume of *HSF* revolves around the social-scientific analysis of process-produced data (a dreadful neologism, coined by Stein Rokkan, which means "all those data, which were collected by public or private organizations in the course of their official duties" [p. 1] and were traditionally known as *Akten*). In his lead contribution on the reorientation of sociology toward history, E. K. Scheuch calls for "the development of a kind of source criticism" of mass files as quantitative data base, which would take their purpose and structure sufficiently into account. In an interesting essay W. Bick and P. Müller explore the organization, utilization, and quality of present administrative bookkeeping in order to contrast bureaucratic images with reality. While other articles contain more conventional advice on legal files, data linkage, sampling, and new sources (P. Borscheid and H. Schomerus use personnel records and marriage/death inventories), the critical evaluation of mass files for quantitative processing and analysis is important enough to deserve further discussion by historians.

The third volume, which originated in a session of the 1976 Mannheim *Historikertag*, offers examples of the application of quantitative methods to specific historical problems. While A. Imhof and T. Kuhn present an excellent introduction into the demographic analysis of "administrative church records," D. Saalfeld has more difficulties with his attempt to portray the evolution of social strata in Göttingen from 1760–1860, since his three census cuts may not be typical, his boundaries in the upper reaches of society are hazy, and he excluded education from his matrix. Although W. Schroeder's investigation of the Socialist Reichstag candidates in the late empire departs from the mistaken assumption that SPSS can handle only a collective analysis, his results are largely convincing. R. Spree's trend and cycle analysis of the economy is one of the first sophisticated forays into econometrics in Germany, while H. Best's study of the trade-policy petitions to the Frankfurt National Assembly in 1848 demonstrates the usefulness of a controlled content analysis, although he sometimes tends to overinterpret his evidence.

Despite the considerable promise revealed in these interesting volumes, one would do well to avoid premature optimism. As Jürgen Kocka stresses in his introductory reflections to volume three, inherent obstacles (the inappropriateness of quantitative methods for many historical questions and the lack of quantifiable sources in others), as well as sizeable costs and continuing traditionalist prejudices, impose firm limits on further development. Although this enthusiasm for the convergence of history and sociology is contagious, one nevertheless needs to remember that mani-

festos, newsletters, and conferences notwithstanding, quantitative methods in German history can prove their worth only by making a substantive and substantial contribution to historical understanding.

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RALPH WALTER QUERE. *Melanchthon's Christum Cognoscere: Christ's Efficacious Presence in the Eucharistic Theology of Melanchthon*. (Bibliotheca Humanistica and Reformatorica, number 22.) Nieuwkoop: B. de Graaf. 1977. Pp. viii, 423.

Ralph Walter Quere's book is a historical work only in so far as it deals with one of the giants of the Reformation. But its content is theology. It will be of primary interest to theologians, especially those working in the field of sacramental theology.

The scholarly contributions are manifold. Quere emphasizes that although the reformers rejected the medieval Church, their sacramental thinking has to be seen against the background of the Church's eucharistic traditions, especially the Augustinian tradition. He explores with fine expertise the stages that Melanchthon's sacramental theology underwent. During the years 1518 to 1521 Melanchthon's thinking centered on the problem of sign and significance; during the period 1521 to 1528 the real presence of flesh and blood and the benefits are the central themes, and after 1530 Christ's efficacious presence in the administration of the Eucharist appear to be the main concern. The major theme in Melanchthon's thinking, then, is the relationship between sign, presence, and benefits.

Melanchthon's views are constantly compared with Luther's evolving position and also with the views of Karlstadt, Zwingli, Bucer, and Oecolampadius. At the same time the author takes issue with the leading German Melanchthon scholar, Wilhelm Neuser.

Contrary to Luther, Melanchthon retained the Augustinian model of *signum-res significata*. Of course, for Melanchthon too, Christ is present in the sacrament efficaciously, personally, and substantially. The thing signified is Christ's will to forgive. The subtle differences with Luther centered not on the real presence but on the mode of his presence. By 1530 Melanchthon had developed a doctrine of personal, substantial, simultaneous, ecclesial, ritual, and efficacious presence, which deviated slightly from Luther's understanding. The difference may partly be explained by the influence of Oecolampadius' *Dialogus* and patristic and medieval thought.

The overriding theme, which Melanchthon conceived as early as 1519, and which provided continuity to his theology and also offers a key to understand his hermeneutic thought, is his concept that *Christum cognoscere beneficia eius cognoscere* (to know Christ is to know his benefits). Or as Quere puts it: "knowing Christ, the true sacrament, is knowing its benefits, to which the signum of the Eucharist points. . . . Though one participates in the very body and blood of Christ, he does not know him if he does not know his benefits through faith" (p. 128). According to Quere, Neuser has not sufficiently recognized and emphasized this fundamental concept.

Melanchthon's originality consists in combining his own views of Christ's personal and ritual presence with the medieval doctrine of *signa efficacia* and Luther's doctrine of the benefits of the sacrament given through the Word to faith, in his own doctrine of efficacious presence. Christ's presence is efficacious and simultaneous, but efficacious only to those who receive it in faith.

Quere's study offers many valuable and suggestive ideas on the evolution of Melanchthon's thought. Surely it will add a new perspective to Melanchthon studies, although so much groundbreaking work has recently been done by German theologians. And yet this erudite and laborious theological dissertation leaves the historian a bit bewildered. One wonders whether it is not easier to read Melanchthon than his interpreters.

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LEWIS W. SPITZ and WENZEL LOHFF, editors. *Discord, Dialogue, and Concord: Studies in the Lutheran Reformation's Formula of Concord*. Philadelphia: Fortress Press. 1977. Pp. xv, 207. \$9.95.

The Formula of Concord of 1577 brought to an end a period of religious controversies which had divided Lutherans in Germany. For thirty years Gnesio-Lutherans had poured down a torrent of invective upon Melanchthon and his Philippists, believing that they were compromising Luther's doctrines in the face of Calvinism and Catholic resurgence, and the Philippists had replied in kind. Leading a small parade of studies marking the quadricentennial of the event is this volume of twelve essays and a companion volume in German, jointly edited by Lewis W. Spitz and Wenzel Lohff.

The present volume is not another survey of the formula's theology, though there are studies of the formula articles treating original sin (Robert Schultz), free will (Ekkehard Muehlenberg), the

Lord's Supper (Ralph Quere), and ceremonies (Oliver Olson). Robert Scharlemann considers the act of confession, and its place within the truth system claimed by religion. Manfred Fleischer and Trygve Skarsten tell why Silesian and Scandinavian Lutherans did not subscribe to the formula, and Robert Preus finds that the Lutheran theologians of the next two centuries made little direct use of it in their work. As W. B. Patterson, W. R. Godfrey, and Jill Raitt explain in their contributions, Queen Elizabeth and her church, the Dutch Calvinists, and the Huguenots opposed the proclamation of the formula because its condemnation of the doctrines of other Protestants could only serve, in their opinion, the Catholic cause. James Megivern examines treatises of Bellarmine and other Catholics against the formula.

No general conclusions can be expected from a book such as this, but significant are themes regarding the German Lutherans, Luther, and Melancthon common to several essays. First, the three articles on the Anglican and Calvinist responses to the formula force a comparison between their unceasing efforts to effect Protestant unity and the Germans' insularity. Secondly, the fact that Bellarmine, the Calvinists, and some Anglican divines attacked Luther's doctrine of the ubiquity of Christ's body, as expressed in the formula, indicates that his Christology was more distinctive than has been generally recognized. To understand Luther here readers need to know more about his nondualistic anthropology than is explained.

Finally, two essays reflect the current reassessment of Melancthon. Since the formula first appeared, several of its articles have been thought to involve repudiations of his later theological position and of his actions after the military defeat of the Lutherans in 1547. Quere demonstrates that his conception of the Lord's Supper was closer to Luther than has been thought, and that specific motifs in the formula's teaching here were distinctly Melancthonian. In discussing free will the formula rejected any notion that the fallen creature participates in his salvation (synergism), and Muehlenberg, for the most part, follows the crowd in assuming that this was a fair rejection of Melancthon's soteriology. Yet in some places Muehlenberg mirrors what Lowell Green has been suggesting: Melancthon was discussing sanctification, not justification, when he said that man's will must act in his salvation. The essay suggests this reassessment, then, but Muehlenberg himself straddles the fence. This reviewer also questions the formula's—and Olson's—assertion that Melancthon and the Philippists after 1547 accepted Catholic ceremonies under the duress of tyrannical rulers. What they set down in the Leip-

zig Interim and practiced was little different from what the Albertine Saxon church had established prior to the war. Therefore, contrary to the formula and Olson, they had no reason to associate liturgy and politics, and rise in resistance to the ruler.

Study of the Formula of Concord has been primarily a Lutheran confessional enterprise. This is an important book because it places the document in a European-wide context of late sixteenth-century theological and political life.

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BERNARD VOGLER. *Le clergé protestant rhénan au siècle de la réforme (1555-1619)*. (Association des Publications près les Universités de Strasbourg.) Paris: Éditions Ophrys. 1976. Pp. 413.

Bernard Vogler's meticulous study of the Protestant clergy of three independent Rhenish states, the Electoral Palatinate, the duchy of Zweibrücken, and the County of Sponheim, reflects the difficulties of condensing a thesis for the *doctorat d'état*. Much of the quality of the original work is lost. The study is based on records of official visitations made by the political authorities to every parish church in these areas at five- to ten-year intervals. The surviving dossiers provide a mine of information on the clergy, their careers, their pastoral activities, their family life, and the level of intellectual and religious culture they were able to maintain.

Vogler relies heavily on quantitative data. There are eighty-six tables and graphs, numbered in the index but not in the text. Several of the tables are difficult to interpret because only numerical frequencies are given and the reader is ignorant of the relevant total. Percentages would be far more revealing. Often too much information is crammed into a single table. The text is essentially an explication of the tables. This leaves Vogler with little opportunity to build up a coherent picture of clerical life or to develop the changes involved in the shifts back and forth from Lutheranism to Calvinism. Would it be better to let the reader interpret the tables for himself and leave the author free to develop a synthesis of the whole?

Vogler's thesis is that the Reformation created a new model for the clergy. The sacerdotal priesthood was replaced by educated pastors sensitive to and dedicated to the needs of their flock. These men were drawn from the professions and from the artisanal ranks. Although in the early years they came from outside the district, after 1590 recruitment was increasingly confined to the local area.

Most attended one of the nearby gymnasiums or universities, with the University of Heidelberg being the dominant intellectual institution. Thereafter, appointment, examination of the new candidate, and his installation were all subject to the authority of the political powers.

What kind of life lay ahead for the newly ordained Protestant minister? His career would usually involve ministry to two different parishes. Marrying within two or three years of his initial appointment, he would bring up a family of three to five children. His pastoral duties were centered on the weekly preaching service and on instructing the young in the catechism. There was little intellectual stimulation. Vogler has made a thorough study of the library inventories of pastors. Each pastor had his Bible, the commentaries of Veit Dietrich, Johannes Brenz, or Luther for the Old Testament, of Brenz or Melancthon for the New Testament. Doctrine was drawn from Melancthon's *Loci Communes* whether the minister were Lutheran or Calvinist. He relied heavily on a sermon collection for his weekly preaching assignment. It was a parochial life in the full sense of the word, modest, restricted, drawn in upon itself.

The reader must put this together for himself. Vogler presents the data from the sources but he has not confronted the implications. How Calvinist was the church after 1583 if only 81 Lutheran ministers departed from 360 parishes? If both Calvinist and Lutheran clergy depended on Melancthon's *Loci Communes* for basic doctrine, were these Phillipist churches? If the parish in these territorial churches had no role in the selection of the ministers, was this a different form of Calvinism? And how did these ministers influence their flocks?

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DAVID E. MUNGELLO. *Leibniz and Confucianism: The Search for Accord*. Honolulu: University Press of Hawaii. 1977. Pp. xii, 200. \$10.00.

In 1697, Leibniz, who had long striven to effect a Catholic-Protestant reconciliation, revealed his vision of a yet bolder ecumenical enterprise. Might not Europe and China join in a global civilizing mission, with China contributing its superior moral philosophy and assimilating in its turn Europe's advanced theoretical science and revealed religion?

Most of the Jesuit missionaries whose writings and correspondence constituted Leibniz's most direct contact with Chinese thought held that only through a blending of Christianity with the native Confucian culture could the conversion of China

be achieved. His ecumenical proclivities led Leibniz both to support this position and to establish affinities with Chinese concepts in his own philosophical system. In their nature and relationships, *li* (principle), *ch'i* (force), and *t'ai-chi* (Supreme Ultimate), as explicated in Leibniz' *Discourse on Chinese Philosophy*, closely resemble, respectively, the monad, *vis viva*, and God. Underlying these similarities, and affording hope of an eventual harmony, was the contemporary theory of the common origin of all human knowledge in remote antiquity, to which a key was available in the hexagrams of Fu Hsi (incorporated in the *Book of Changes*) which, again, are closely related to the elements of Leibniz's binary arithmetic.

Perhaps David E. Mungello's book is best understood as an attempt to solve two problems implicit in the foregoing summary. The first, whose investigation was urged by Joseph Needham in his *Science and Civilisation in China*, is whether Leibniz's influential organicism was derived basically from Chinese sources. Mungello demonstrates convincingly enough that Leibniz found in Chinese thought not original inspiration but corroboration for his previously and independently developed notions. He further argues that some important distinctions escaped Leibniz because he identified Neo-Confucian texts such as the *Compendium*, upon which he especially relied, with the original Confucian classics. But his own explanation of the correspondences in question is unsatisfactory, resting upon an otherwise undefined "similarity of needs or common interior human structure" (p. 16).

Graver difficulties attend Mungello's response to the second principal problem posed by the study. Why did the "search for an accord" fail? Ultimately this is traced to the subordination of the moral to the severely intellectual in the philosophy of Leibniz, who, with his shallow conception of religious experience, could neither appreciate nor accommodate the element of spiritual cultivation in the Chinese world-outlook—a quality Mungello says we must capture if we are to approach an accord in our own day. But it was precisely by means of his search for God through knowledge that Leibniz was able to transcend the faith-reason dilemma in seventeenth-century European thought—a tension with parallels in Chinese philosophy. An understanding of Leibniz the man, which emerges nowhere in this book, might temper the inclination to portray him as simply a cold rationalist. After all, the steam had gone out of both the Catholic-Protestant and the East-West ecumenical movements before the *Discourse* was written, and for reasons unrelated to its content. Already in the eighteenth century, some European thinkers realized that mutual enrichment was

more likely to result from a humane relativity than from the delineation of a hypothetical common ground. Perhaps the lesson to be learned from Leibniz's "failure" is the necessity of recognizing the sober limits of the possible.

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REINHARD MÜTH. *Studentische Emanzipation und staatliche Repression: Die politische Bewegung der Tübinger Studenten im Vormärz, insbesondere von 1825 bis 1837*. (Contubernium, Beiträge zur Geschichte der Eberhard-Karls-Universität Tübingen, number 11.) Tübingen: J. C. B. Mohr. 1977. Pp. xviii, 298. DM 47.80.

"University history generally speaking does not have the reputation of being very attractive," Johannes Haller declared while he was writing a history of the origins of the university at Tübingen in the 1920s. "Too frequently it is a small, often dull mirror, reflecting only a limited part of national life." Gerhard Ritter challenged Haller's assessment, claiming that, on the contrary, university history "might at any time extend one's outlook from the local and provincially limited region over national to universal boundaries." Both historians confined their expectations of academic interaction with other aspects of society to the sphere of politics. The editors of *Contubernium*, a series devoted to the university of Tübingen in which Reinhard Müth's volume appears, specifically expressed the hope that the monographs they sponsored would transcend the dimensions of the Haller-Ritter debate and explore the social implications of university development. Unfortunately, *Studentische Emanzipation* exemplifies Haller's apprehensions. Müth offers an excellent descriptive account of the interplay between student activity at Tübingen and government repression from Stuttgart, but he deals only tangentially with the relationships between the Tübingen *Burschenschaft* and the movement at other German universities; he makes no comparisons, and he fails to address more general issues, such as generational tension. Social and economic inference are ignored in deference to politics.

The *Burschenschaft* at Tübingen resembles the general movement except in its initial phase. Because King Wilhelm I considered the Carlsbad Decrees a violation of Württemberg law, students at Tübingen enjoyed a period of relatively unimpeded organization until 1825. Thereafter, suspicious of the conspiratorial *Jünglingsbund*, Stuttgart dispatched a special commissioner to supervise student affairs and reduced the institutional autonomy of the university itself. Repression, however, politicized the Tübingen *Burschenschaft*. In-

spired by the Polish example, students extended support to the opposition press, and to the radicals at Hambach, as well as in the Stuttgart Landtag. The failure of the *Wachensturm* at Frankfurt in 1833 destroyed their political hopes and provoked the treason trials which resulted in the academic expulsion of thirty-seven Tübingen students. The amnesty occasioned by the king's jubilee excluded students, who were required to pledge their political passivity as a condition for matriculation. After years of political incubation, Tübingen students succumbed to the divisiveness of the revolutionary opposition in 1848, according to Müth, and the radicals among them, disciples of David Friedrich Strauss, experienced bitter reprisals for their role in the Baden uprising.

Two episodes of particular interest interrupt this narrative. In 1831, when the artisans and vineyard workers rose against the *Landjägerpolizei* in Tübingen, and again in 1847 when the local populace staged a bread riot against the millers and bakers, the *Burschenschaft* organized a student militia to keep order, interposing themselves between the townspeople and police from the central administration. By this strategy of apparent loyalty, according to Müth, the students succeeded in earning privileges from the government. Their inability to take the part of their fellow citizens, however, betrayed their elitism and foreshadowed the liberals' incapacity to cooperate with popular democratic forces in 1848. Müth suggests that the social gulf between students at Tübingen and local artisans precluded collaborative effort, an insight he should have developed. No information whatever is offered about the class origins of the students.

Müth's bibliography confirms the impression of a book diligently researched at the local archival level but flawed by the absence of a more general perspective. Like many German academics, Müth is apparently unfamiliar with American work in his field, such as the chapter on German student unrest by Konrad H. Jarausch in Lawrence Stone's *The University in Society*, or the scholarship of Rolland R. Lutz. The appendixes of Müth's volume, however, almost justify its narrow focus, reproducing a treasury of illustrations, songs, and other memorabilia from *Vormärz* Tübingen.

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REINHARD SPREE. *Die Wachstumszyklen der deutschen Wirtschaft von 1840 bis 1880 (mit einem konjunkturstatistischen Anhang)*. (Schriften zur Wirtschafts- und Sozialgeschichte, number 29.) Berlin: Duncker and Humblot. 1977. Pp. xxx, 577. DM 60.

Reinhard Spree's book amounts to the most stimulating inquiry into Germany's "take-off" to come

along in many a year. The study of German growth cycles had lain dormant since the work of Rosenberg, Schumpeter, Spiethoff, Wagemann, and Varga in the thirties. Then, in the postwar period, it revived in the form of W. G. Hoffmann's magisterial time series. Now Spree has expanded on these foundations by matching the fluctuations of the aggregate economy with those of its sectoral components in an attempt to lay bare the "base mechanism" of the four most crucial decades of German economic growth.

Spree very wisely eschews the rigidities of an econometric model and pursues his inquiry through a theory-based but more flexible, quantitative macro-analysis. Specifically, he seeks to measure the dimensions of key economic variables (indicators) so as to identify the stages of aggregate and sectoral cyclical growth. Not unlike Schumpeter and Mottek, he finds the prime mover behind the economy's fluctuations in the autonomous impulses and backward linkages of the investment cycles which accompanied Germany's great railroad booms. Similarly, he offers qualified economic support for Borchardt's "Dualismus der Uebergangsgesellschaft." And he has compiled what must surely stand as the most comprehensive collection of time series on Germany's economic fluctuations (1840-80) available today.

Yet when it comes to explaining the growth patterns of Germany's "take-off" in their concrete totality, the limitations of a strictly quantitative approach begin to tell. A shortage of data precludes the substantiation of significant linkages between the aggregate economy and its sectors or between individual sector "blocks." Indicators for crucial theoretical variables cannot be fashioned while qualitative inputs, especially from contemporaneous sources, are ignored. Spree therefore concludes with generalizations which, in his own words, often read too much into fragmentary data and qualify as hypotheses at best.

Thus Spree seems to reassert both the primacy of demand over supply in the development equation and of changes in the real sector over the initiative of banks. From there he proceeds to more specific claims. He not only postulates that the impact of the world economic crisis of 1847 and the revolutions of 1848 on Germany's nascent economy was negligible and that the amplitude of the "Gruenderboom" of the 1850s clearly exceeded that of the 1870s, but he also believes that the wars of 1864, 1866, and 1870/71 put the brakes on German economic growth, that the impact of the French reparations on the boom of the early seventies has been exaggerated, and that the financial crisis of 1873 played less than a decisive role in that boom's collapse.

Ironically, the powerful device available to the

Marxians, of reconciling the concrete tendencies in an economy with the inner logic of an abstract model of societal change, has been blunted under the stagnant subjectivism of "*kapitalistische Krisenforschung*." Spree has opted for an analysis of "capitalistic" growth under the conditions of Gerschenkron's "great spurt," which necessarily occurs in cycles. He has acquitted himself well. His updating and editing of economic data has done more than redefine the key variables and their movements in the German economy in its "take-off" phase. Spree has reminded us once again how tenuous our comprehension, and how variegated and ultimately unpredictable the process, of economic growth really is.

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HORST MÜLLER-LINK. *Industrialisierung und Aussenpolitik: Preussen-Deutschland und das Zarenreich von 1860 bis 1890*. (Kritische Studien zur Geschichtswissenschaft, volume 24.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 506. DM 92.

Horst Müller-Link's reinterpretation of Bismarck's Russian policy follows the Fritz Fischer school in analyzing diplomacy as a function of economic factors and the resulting social and political tensions. Citing significant new evidence from German embassy and consular papers and the records of agrarian and industrial interest groups, Müller-Link traces the progressively developing conflict in German-Russian relations. Russian economic development following the Crimean War depended on increasing agricultural exports and imports of capital, technology, and machinery. The more advanced industrialization of Germany created a community of economic interests between a developed and a developing country, for which the commercial policies of the liberal era were well suited. The defense of the existing social order in the face of rapid modernization, however, propelled both countries into intensely nationalistic policies which undermined the relationship and transformed European diplomatic alignments.

The decisive factor provoking German-Russian economic rivalry was the agricultural price decline of the late 1870s, which rapidly converted Prussian landowners from free trade to protectionist doctrine. Müller-Link shows that anti-Russian agitation played a prominent part in the tariff campaign that welded political support for the alliance of iron and rye. Since the tariff provided a weapon for forcing open the Russian market for German manufactures and offered an excuse for the reform of Reich finances, foreign policy became the means to consolidate domestic political power.

Russia's exclusion from the London capital market weakened Czarist credit at a time of acute agricultural crisis. Under pressure from the aggressively nationalistic Moscow bourgeoisie, Alexander III attempted to strengthen the autocratic regime by a policy of social reaction and military expansion. In spite of its anti-German elements, this course led to increased Russian dependence on German loans. Although he recognized in May 1882 that "sound Russian finances mean war" (p. 250), Bismarck welcomed the resulting leverage over Russian policy. With the escalating tariff conflict and continuing increase of armaments, the General Staff advocated a preventive war to avert the potential nemesis of a two-front war. Bismarck preferred military threats and economic sanctions, including blocking Russian access to the Berlin financial market, in an attempt to control the direction of Russian policy. The resentment created by such tactics, which turned into a full-scale economic war, fueled the rising flames of Russian nationalism, and the French loan of 1888 offered the Czar an escape from such intolerable subservience to Germany. Thus Bismarck's policy hastened the outcome he dreaded most: Franco-Russian rapprochement and the division of Europe into two hostile armed camps.

While many readers will regard some of the props of this argument as unproven, and perhaps unprovable, Müller-Link's identification of the structural elements affecting German-Russian economic relations adds another dimension to the discussion of continuity in German history. Since the substantial basis for trade has survived profound political changes in both countries, the interplay of convergent economic interests and divergent political interests has been a recurrent feature of their relations for over a century.

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ALEX HALL. *Scandal, Sensation and Social Democracy: The SPD Press and Wilhelmine Germany, 1890-1914*. New York: Cambridge University Press. 1977. Pp. ix, 267, \$18.95.

This is a somewhat ragged work whose focus remains unclear and which fails to elicit significant conclusions from its quite interesting material. Is it, as the subtitle suggests, a portrayal of the SPD press in the years specified, or an analysis of its role? No, not really; anyone with first-hand acquaintance with Socialist newspapers of that era will learn little here that is new. The main title, unrevealing as it is at first, actually encapsulates the substance of the book better. We have here a

discussion of the gross and repellent side of Wilhelmine society—"the cancer of corruption and the injustices which flowed from an illiberal political tradition" (p. 188)—loosely tied to an account of how the SPD reacted to these abuses, particularly through its press.

The socialist movement appears in Alex Hall's pages mainly as a political object, at most as a reacting force. Here lies the principal merit of the book: its portrayal of the gross chicaneries to which the socialist movement was subject is a powerful reminder of one of the most important factors—perhaps the most important factor—enforcing the cohesion of the party and its continued radical demeanor. The material upon which the author draws is imposing, including police and political files from over a dozen West German archives, and the presentation is fluent and readable. One can only wish that this wealth of material had been made to yield clearer results. The beginnings are there, for instance, where the author opens a chapter with the resounding claim: "The SPD press and indeed the Social Democratic movement as a whole were only as radical and effective as the authorities allowed them to be" (p. 41). This assertion touches on one of the key analytical problems for historians of German socialism, and suggests still others, but none of them are developed.

Ultimately, the author appears to be interested less in the SPD as such—he has consulted very little of the available mass of private papers of SPD leaders—than in the relations between government and SPD as indicators of the distortions and oppressiveness of Wilhelmine society. The indictment (which is what it is) is powerful; according to Hall, the infamously politicized justice of the Weimar years grew directly out of the badly distorted justice of the Kaiser's times. Unfortunately, the presentation is anecdotal and impressionistic, though this is not for lack of material. The book shows very wide reading on the part of the author, and a number of the scandals it recounts receive their first or their best treatment in English. The writing, however, is neither as careful nor as precise as it should be. More importantly, there is no connecting thread. Certainly, the scandals and the confrontation of SPD and state reveal much about the attitudes and inner structure of the society, but in this book they are made to reveal not one or two major insights, but rather many particular things, and these often not new. It even seems as though obvious themes are bypassed; we are repeatedly told, for instance, that the Social Democrats regarded the scandals detailed here as symptoms of disintegration, but the author never comes to grips with this assertion.

For the reader with an interest in the book's

subject matter, the work can be recommended. It is written with attractive energy, reads quickly, and conveys valuable information. But the reader will have to interpret the material for himself.

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BERND F. SCHULTE. *Die deutsche Armee, 1900-1914: Zwischen Beharren und Verändern*. Düsseldorf: Droste Verlag, 1977. Pp. xxxv, 591. DM 68.

Despite the extensive literature on the causes of the German defeat in the First World War, little has been written about the actual quality of the German army. Historians have, to be sure, concerned themselves with the size of the force that was mobilized in 1914 and its relation to the strategic plan devised by the General Staff, and it has been suggested that the strength of the army was affected by the high naval expenditures before the war, as well as by the War Ministry's reluctance to push for expansion, because of the deleterious effects it would have on the social cohesiveness and political reliability of the officer corps. But less attention has been paid to the education and training, tactical doctrine, and technical efficiency of German troops as they took the field. Bernd F. Schulte's book, which is based on very extensive research in military collections in Munich, Stuttgart, and Freiburg, as well as the archives in Koblenz, Karlsruhe, and Buckeburg, is an ambitious attempt to correct this deficiency.

Schulte's general point is that until the very eve of the war the German army was, as the English critic Repington wrote in the *Times* in 1911, "living on a glorious past." Its training programs were traditional and formalistic, reflecting little awareness of the way in which the art of war had changed since 1870 and emphasizing discipline and drill and school exercises that were often grotesque in their remoteness from reality. Its tactical doctrines were uninfluenced either by the cogent criticisms of foreign observers, which Schulte analyzes at some length, or by the lessons of contemporary wars like those in South Africa, the Far East, and the Balkans, which German soldiers, as the author points out, largely misread. They were characterized as a result by an underestimation of modern firepower, a scorn of terrain appreciation and camouflage, and a persistence in outworn forms of offensive thought, like shock tactics, the deployment of artillery in open formation, and the use of massed, instead of dispersed, infantry attack. Finally, it showed great reluctance to accept technical innovations, and was very slow to adopt the machine gun, to begin to develop an air force, and to acquire experience in the use of combined arms.

Schulte is very good at describing all this but less so in explaining the reasons for it. Because his book lacks a comparative dimension, and because he has not apparently read C. E. Montague's *Disenchantment* or other works of that kind, he fails to see the obvious reason, which is, as World War I demonstrated, that all army commands in the pre-war years were conservative and opposed to innovation and failed to prepare properly for the war that was coming. That kind of explanation is not good enough for Schulte, who blames the shortcomings of the German army on its preoccupation with the threat of domestic revolution and the necessity of being prepared to deal with a civil war. This is the kind of argument that fits current fashion, but unfortunately, although he tries valiantly, Schulte never succeeds in showing exactly how concern over the possibility of a general strike in Hamburg determined the tactical doctrines and forms of the field army.

GORDON A. CRAIG
Stanford University

BIRGIT KNORR. *Autorität und Freiheit: Das Liberalismus-Verständnis des Bildungsbürgertums im Kaiserreich und in der Weimarer Republik im Spiegel der Historiographie über den Frühliberalismus (1815-1848)*. (Europäische Hochschulschriften, Series III, Geschichte und ihre Hilfswissenschaften, number 78.) Bern: Peter Lang, 1977. Pp. 233. 45.00 FR.

Considering the historical profession during the imperial and Weimar periods "the embodiment of the intellectual and political attitudes of the educated middle class," Birgit Knorr attributes to historians an "immense influence" on the prevailing values in bourgeois society (p. 8); she concludes that the failure of German liberalism must, to a considerable extent, be laid at their door. In her opinion, historians should have offered a deep and thorough critique of Prussian authoritarianism, Bismarck's *kleindeutsch* solution to national unity, and the Prussian-inspired revolution from above that left political power in the hands of an anachronistic elite while stimulating social and economic modernization. Instead, the historiography of early liberalism in the *Vormärz* (the period in which German liberalism departed from Western, Anglo-French liberalism) constituted both a glorification of Bismarck's authoritarian empire and a justification of the illiberalism of the educated middle class. "The historical writing of the empire and the Weimar Republic alienated itself from its liberal origins, renounced the realization of political self-determination, satisfied itself with a legalistic guarantee of individual freedom, formalized moderate early liberalism's

contradictory synthesis of freedom and authority, and emptied it of its emancipatory content" (p. 9).

Unfortunately, the approach Knorr chose greatly reduces the value of the book, which is based on the interpretation of early liberalism in the work of thirty-two historians. So that opinions on all main issues could be compared, she chose only those historians who wrote about the entire *Vormärz* period. But in so doing, Knorr restricted her study to general histories of Germany, Prussia, or, in a few cases, the world. Biographies and regional studies, which constituted much of the most important work of this period, are excluded. Moreover, the reader is asked to accept the representativeness of the works selected without broader consideration of the intellectual biographies of the historians in question, or any concrete evaluation of the impact of these historians on their middle-class audience. Of the works studied, all but seven were published prior to the founding of the Weimar Republic, and by far the most attention is given to Treitschke. Yet, as Knorr points out on several occasions, the conclusion that Treitschke's views on early liberalism reflected his pro-Bismarckian patriotism is hardly new or surprising. The only Weimar historian examined at length, and the only one whose work is accorded Knorr's approval is Schnabel, though he too does not receive perfect (liberal) marks on all issues. In short, Knorr does not make a convincing case that the histories and historians studied are representative of the profession, of academia, or of the educated middle class, and this least of all for the Weimar years, when the failure of liberalism led to catastrophe. The value of the book lies in its concise summary of a variety of interpretations of early liberalism, not in any new insights into the relationship between politics and the writing of history.

JEFFREY M. DIEFENDORF
University of New Hampshire

JOHN A. LEOPOLD. *Alfred Hugenberg: The Radical Nationalist Campaign against the Weimar Republic*. New Haven: Yale University Press. 1977. Pp. xvi, 298. \$17.50.

Alfred Hugenberg remains, even after this admirable study, a baffling figure. At a time when politicians generally beat their big drums, he stayed for years in the background. Raw power without external trappings seems to have obsessed him. Hugenberg was, according to Heinrich Class, the true founder of the *Alldeutscher Verband*. Before 1914 he moved in and out of the Prussian bureaucracy, becoming an expert on eastern Germany—strongly anti-Polish—and formed a small circle of

loyal and able friends. He became chairman of Krupp's board of directors in 1909, rapidly developed close personal connections with such major figures in Ruhr heavy industry as Emil Kirdorf, demonstrated his skill in managing funds and producing discreet propaganda for the chief business organizations in the Ruhr, and topped off his work in publicity by the grand coup of acquiring the Scherl press holdings in 1916. During the Weimar Republic he became the leading purveyor of information for the Right through newspapers, magazines, films, the *Telegraphen-Union* news service, and various firms "advising" provincial newspapers.

During the war Hugenberg worked with the *Alldeutschen* in their campaign against Chancellor von Bethmann-Hollweg and a soft peace. Late in 1918, when conservatives, *Völkische*, and rightist agrarians founded the *Deutschnationale Volkspartei* (DNVP), he became a member and served inconspicuously in the *Nationalversammlung* and the Reichstag, at the same time pursuing his business interests. Not even the Dawes Plan crisis that shook the DNVP in 1924 brought Hugenberg before the public. But Class and a few other friends decided to build him up politically when members of the DNVP, perpetually infighting, disagreed about the Locarno treaties and participation in the Reich government. The arts of quiet negotiation and his innumerable private connections outside the Reichstag *Fraktion*, served Hugenberg well. Surprisingly, when the party's vote declined seriously in the 1928 elections, he was chosen its new chairman. "*Block nicht Brei*" was his motto—better a small, determined, and intransigent following than a large, quarreling mass.

Secession followed secession in the DNVP, yet Hugenberg retained his place and his policy, viewing the party as the potential core of a *Sammlung* of the bourgeois Right against the republic and Marxism. He failed to achieve such a coalition of forces, of course, even though he held several ministries in the first Hitler cabinet, when experts thought the conservative majority would box in the Nazis. Hugenberg objected to the terror of the new masters. Ironically, his intense agrarian-autarkic activities as minister led to his estrangement from other non-Nazis in the cabinet and to his resignation in June 1933. His last official words to the remnants of the DNVP informed them that they should not believe any reports in the future that he had committed suicide. Nothing in his political career became him like the leaving of it.

After the war Hugenberg was cleared of the charge that he had been a major contributor to the Nazi acquisition of power in Germany. It is difficult to disagree with the author's conclusion that: "Up to his death, Hugenberg refused to admit his

responsibility for the development of Nazism; history must disagree."

John A. Leopold uses well the Hugenberg *Nachlass*, which is splendidly organized at the Federal Archives in Koblenz, as well as the papers of Hugenberg's friends Leo Wegener, Schmidt-Hannover, and Gottfried Traub. Further important sources include the Tirpitz papers in the Freiburg Archives, the *Deutsches Zentral-Archiv* in Potsdam, industrial archives (especially the *Gutehoffnungshütte*), and private collections, such as the papers of Count Westarp. Leopold is fully familiar with the published materials, primary and secondary, and uses them critically. Something more on the activities of the DNVP in South Germany, beyond the passages on Traub and the *München-Augsburger Abendzeitung*, would have been welcome; so would fuller treatment of the influence of the *Alldeutscher Verband* in building up Hugenberg as a political force after 1926. A word of criticism: if we must have hundreds of footnotes assembled in a mass separated from the text, we need an alphabetical bibliography, not simply a bibliographical essay dealing only with archival material. And we need more careful editing and proofreading of the text.

REGINALD H. PHELPS
Harvard University

HANS SPEIER. *Die Angestellten vor dem Nationalsozialismus: Ein Beitrag zum Verständnis der deutschen Sozialstruktur, 1918-1933*. (Kritische Studien zur Geschichtswissenschaft, volume 26.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 202. DM 28.

Studies on the history of white-collar employees are more numerous for Germany than for other countries, partly because white-collar employees were an important source of mass support for the Nazi movement and partly because the peculiar structure of German society has strongly attracted the interest of social scientists in the German lower middle class since before 1914. A number of books and essays have been published recently on the social history of German white-collar employees. Hans Speier's study is one illustration of the renewed interest in this topic.

The book is peculiar in its own history and in the answers it presents. It is the revised version of a manuscript, written in the last years of the Weimar Republic, which shows the high standard of empirical German social sciences destroyed by the Nazis. The study was not published in 1933 as intended, because of the Nazi seizure of power. Only recently, under the encouragement of Jürgen Kocka (one of the specialists on white-collar history), has Speier published the manuscript, about which specialists already knew.

The study by Speier combines social history and political history. The book has three main parts: an economic section, a section on social prestige (or rather *soziale Geltung*, as opposed to *Sozialprestige*), and a section on white-collar organizations. In the first section, Speier points to the strong heterogeneity of white-collar employees, describing the differences in the work situation, skill level, social origins, chances to become independent, and the social status of various white-collar occupations, including clerks, salesmen and saleswomen, factory masters, and technicians. Speier's conclusions differ from those in other studies that stress the common socioeconomic situation of white-collar employees to a greater extent.

In the second part, Speier deals with the common characteristics which, in his view, can only be found in the area of social prestige, or—to put it more exactly—in the characterization of the white-collar workers by themselves and by other groups in interwar German society. According to Speier, the prestige of German white-collar employees depended mainly on their close relationship to employers, an educational level higher than that of blue-collar workers, and on their nationalistic attitudes, and was not related to their socioeconomic situation. Once again, this assessment is not corroborated by other recent studies, or if so, only with incisive modifications.

In the third section, Speier describes the development of white-collar organizations since the late nineteenth century, dealing with the shift to the left in 1918, long-term unionization, and the strength of the various wings. He does not examine in detail white-collar attitudes toward the emergence of Nazism, nor does he account for the easy take-over of white-collar organizations by the Nazis after 1933. In this respect, the somewhat ambiguous title might lead the potential reader astray as to the contents of the book.

Unfortunately, Speier does not develop the relationship among his three main themes—socioeconomic situation, social prestige, and political attitudes of white-collar employees. Nevertheless, though the core of the book was written almost half a century ago, it is not obsolete, but is still a valuable contribution to the social history of the Weimar Republic; the basic questions are those asked by researchers today, and the book contains material which otherwise would be inaccessible or extremely difficult to collect. *Die Angestellten* puts forward, though in the language of interwar sociology, many stimulating ideas and arguments.

HARTMUT KAEUBLE
Free University of Berlin

UWE DIETRICH ADAM. *Hochschule und Nationalsozialismus: Die Universität Tübingen im Dritten Reich*.

With a supplement by WILFRIED SETZLER, "Die Tübinger Studentenfrequenz im Dritten Reich." (Contubernium, Beiträge zur Geschichte der Eberhard-Karls-Universität Tübingen, number 23.) Tübingen: J. C. B. Mohr, 1977. Pp. x, 240. DM 37-40.

This absorbing study of the University of Tübingen is essential reading for anyone interested in German universities during the National Socialist era. Although German universities offer no typical example, the case of Tübingen is particularly suggestive. Before 1933, the faculty was overwhelmingly conservative and nationalist. There were only a handful of republican supporters. With the exception of one *Extraordinarius* who left in 1931, Tübingen had no Jewish faculty members; there were few Jewish students. Duelling fraternities were probably stronger here than at any other university in Germany.

During the early years of National Socialist power the conservative, "Aryan" character of Tübingen's faculty cushioned the university from the disruptions experienced elsewhere. More than a few faculty members greeted the new regime warmly. Yet tensions between a university community wishing to maintain its traditional autonomy and a political movement hostile to the idea of scholarly and scientific independence were not long in surfacing.

Uwe Dietrich Adam maintains that the University of Tübingen successfully weathered the twelve years of the Third Reich. To be sure, the years of Nazi rule meant interference and control by the Reich government and Nazi Party, imposition of the dictatorial Führer principle of organization at the university, a drastic decline in student numbers, regimentation of student extracurricular life, and continuing pressures from party ideologues and opportunists. On the positive side, Adam emphasizes, the theological faculty remained open in spite of efforts to close it, and scholarly accomplishment as well as political leanings and personal contacts remained a factor in university appointments. Indeed, the collegial organization of the individual faculties was not totally suppressed by the Führer principle, and the status of the lower ranks actually improved. Adam also marshalls evidence to demonstrate that the period was by no means barren in research. Nor did research and instruction of the specifically National Socialist variety ever become a major factor in Tübingen.

Adam argues that the survival of the university during this period was made possible by the faculty's will to resist that extended even to certain key figures who owed their positions to National Socialist rule. While the evidence he offers can be used to defend this position, it is difficult to ignore

the continuous compromises that made possible the small victories he emphasizes. While he effectively refutes the "brown university" label, there is little question that academic freedom as we know it ceased to exist. In this context, diplomatic skill and an understanding of the limitations of Nazi rule were more essential and more commonplace than courage.

Adam's sober and frank accounting of the careerism and politicking that characterized this period humanizes the actors and universalizes what happened in a way that should disquiet other academic communities. The book is carefully researched and effectively organized in both topics and periods. It is gracefully written and holds the reader's attention throughout.

MICHAEL STEPHEN STEINBERG
Institute of European Studies

HANS UMBREIT. *Deutsche Militärverwaltungen 1938/39: Die militärische Besetzung der Tschechoslowakei und Polens.* (Beiträge zur Militär- und Kriegsgeschichte, number 18.) Stuttgart: Deutsche Verlags-Anstalt, 1977. Pp. 296. DM 48.

The price of military victory is the chore of ruling one's erstwhile opponents. No matter how extensive the planning may have been, the actual administration of a conquered people must rely on ad hoc measures to deal with the most pressing necessities of the moment. The task of actually putting decisions into effect amidst a hostile population is complicated immeasurably by the absence of a loyal native bureaucracy. The notion that military victory confers supreme power is therefore a delusion. In so revolutionary an upheaval, events take their own course, and no one exerts the amount of guidance and control possible in more stable times. Such might be the broadest generalization if one attempted to universalize on the basis of the German occupation of Czechoslovakia and Poland in 1938 and 1939. Whatever its worth, however, it would not exhaust the total contribution to historical knowledge of Hans Umbreit's book.

To begin with, Umbreit points out that Nazi Germany had no unified plan for the administration of occupied Poland. On the contrary, there were two basic and competing directions in German policy. One emphasized the economic exploitation of Poland and found considerable support within the Wehrmacht in particular. The other, growing out of Nazi ideology, aimed at "the forceable suppression, and spiritual as well as physical weakening of Poland"; both the party and the SS were willing to accept economic losses in realizing that goal (p. 273). Although Hans Frank as governor general tended gradually to

ward the first and more pragmatic course, the competing claims of economic exploitation and ideological spleen-venting wrestled for dominance throughout the German occupation of Poland. This tension between rival policy claims—and the inability ever to resolve or harmonize them—illustrates again the muddle that was Nazi policy and administration in general.

The subtitle of the book would be misleading if the reader were to infer that the book provides equally serious treatment of Poland and Czechoslovakia. Such is not the case. Umbreit directs attention to the German occupation of Poland almost to the total neglect of Czechoslovakia. One short chapter treats not only the occupation of Bohemia and Moravia, but the Anschluss of Austria as well. This disparity reflects Umbreit's interests, but it also illustrates the fact that German authorities found Poland considerably more difficult to rule than Czechoslovakia. The Czechs were not so passive as Umbreit's silence might imply, but clearly the Poles offered much greater challenges. The German documentary sources upon which Umbreit relies are therefore significant for comparative Central European history—as much for what they do not say as for what they do.

The book is written by a specialist for other specialists. A highly detailed work on a very short period of time, it is heavily footnoted with material from German military and administrative archives. Umbreit's almost exclusive reliance on German sources further constricts the study.

The book is a volume in a series published by the Military History Research Office in West Germany. Umbreit's interpretation emphasizes the difference between the traditionalist attitudes toward Poland held by the German military and the more simplistic and horrifying notions of the Nazis. That distinction has to be comforting to those who would disassociate the German military tradition from the Third Reich.

Deutsche Militärverwaltungen 1938/39 does constitute a respectable monographic contribution to German military history. Given its subject matter, sources, and approach, however, it is likely to arouse considerably greater interest within Germany than abroad.

F. GREGORY CAMPBELL
University of Chicago

DAVID DOWNING. *The Devil's Virtuosos: German Generals at War, 1940/5*. New York: St. Martin's Press. 1977. Pp. 256. \$10.95.

With the brisk business in books on World War II and the remarkable disclosures made possible by

newly released documents, the reader can look forward to new publications, even those with such tendentious titles. To begin with the good news, author David Downing displays a remarkable gift for metaphor and historical comparison. German generals are compared to a Genghis Khan at one point and to a Spencer Tracy role in the film *Northwest Passage* at another. He also compares the virtuoso, and personally virtuous, generals in the service of the devilish Hitler to the Biblical Pharisees, who also avoided seeing a suffering humanity. This comparison is less likely to raise eyebrows than comparing American racism in Vietnam to Hitler's in Russia, and in comparing the name given to Vietnamese as "gooks," to the term applied to Slavs as *Untermenschen*.

The title reveals the author's hang-up in contemplating "the finest thinking and fighting soldiers of modern history," who somehow serve the cause of "calculated barbarity without recent parallel." The latter generalization requires less justification than the former, and it is unfortunate that the author does not delve more deeply into the skills of Bock, Brauchitsch, Guderian, Manstein, Model, Rommel, *et al.* What he has accomplished instead is the selection of interesting passages from their memoirs and the weaving of such threads into relatively simple narratives. Yet with the possible exception of the point made about the possibilities of defensive Blitzkrieg (pp. 24–25), there is little that has not been described many times before.

For the purposes of students interested in a simple story and provocative prose, the author has provided a service, but even they would be well advised to look for materials presented with more profundity and less traditionalism in interpretation. If one considers the very limited bibliography, consisting of the major German memoirs in translation and the older, standard British surveys, one must wonder why the author did not read documents, other memoirs, or even the really remarkable secondary works which have appeared in the last ten years. He treats the Moscow campaign with no apparent awareness of the very relevant views of Barry Leach, who suggested that the disobedience of fallible generals was part of the problem. Instead the Moscow defeat is explained by using the old standbys: the bitter cold, the spy Sorge, and the Siberian troops. The Normandy campaign is discussed as though the Ultra secret and the operation, "Bodyguard of Lies," did not exist.

By choosing to cover the complex, six-year war in Europe in only two hundred fifty pages the author may have condemned the result to superficiality. It is to be hoped that he will apply his enthusiasm and his gift for words with more in-

tensity to certain aspects of the war and fashion a more incisive analysis.

The author is not the first, nor likely the last, to be disturbed that the superb intelligence and skills of a Manstein or Guderian furthered the aspirations of the half-mad Hitler. Perhaps true historical perspective will provide the awareness that men, personally virtuous, are frequently found serving irrational leaders of other countries and other causes, where the obsession is power and the nightmare is defeat. It is a lesson of history that virtue, linked with ambition, enables vice, linked with power, to survive.

E. N. PETERSON
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JOSEPH GOEBBELS. *Tagebücher, 1945: Die letzten Aufzeichnungen*. Introduction by ROLF HOCHHUTH. (Joseph Goebbels Tagbücher, 1924–1945.) Hamburg: Hoffmann and Campe. 1977. Pp. 607. DM 36.

The appearance of Goebbels's diaries for the year 1945 comes as a major publishing event, leading to a deeper understanding of this diabolical yet brilliant man. They reveal a very complex individual consumed by the passion of the historical drama in which he found himself. If victory could not be forced from the jaws of defeat, he was determined to stage an epic theater of annihilation.

The diaries offer further evidence of Goebbels's devotion to Hitler and his mission. On the other hand, they reveal his disappointment with Hitler, which no earlier documents demonstrate in such a dramatic way. Thoughts such as "if I were the Führer" arose again and again. For example, Goebbels pressed for the removal of Göring from command of the Luftwaffe as punishment for his "betrayal" of the German people. Each successive devastating bombing attack—often featuring raids of 1,100 bombers—incited renewed criticism of the Reich marshal, whose deficiencies of command and decadent life style became an obsession for the propaganda minister. Yet Hitler refused to take resolute action against him.

Further, he allowed far too many "unheroic toadies" to continue in leadership positions at a time when the Reich lay in its death agony. Goebbels found Bormann totally inadequate to the demands of his position. Ribbentrop had long ceased to act in the manner of a foreign minister, and what he did undertake fell ridiculously short of the necessary measures. Rosenberg—who remained as head of the Ministry for the Occupied Eastern Territories at a time when the Soviets had long since reoccupied them—was beneath contempt

Speer was becoming soft when the situation demanded strength beyond human strength. Most German officers disappointed Goebbels as well, with the notable exception of General von Schörner. He was gratified, however, when Hitler dismissed his rival at the Führer Headquarters, Reich Press Chief Dietrich.

With German fortunes in the war at their nadir, Goebbels implored Hitler to speak directly to the German people. Such an appeal, the minister was certain, would be equivalent to a major victory on the battlefield. Curiously, Hitler refused this request, a situation that troubled Goebbels very deeply.

Even in the bitter year 1945 Goebbels could not forego his limitless vanity. His poorly received speech to the nation in February had been "excellent." He quotes London sources hailing him as a propaganda genius. As late as March he was editing proofs of yet another volume of his collected works. After lamenting the burning of the Propaganda Ministry, he took solace in Hitler's promise that after the war a monumental new Propaganda Ministry building would be constructed for him.

Goebbels's hatreds raged as never before in those final weeks of the war. As for the Jews who remained, they "have to be slaughtered like rats, when we are in a position to do it. Thank God we have taken care of this root and branch in Germany." He blamed the war weariness of civilians in Germany's western territories on "poisonous" French influences. In contrast, the Germans in the "more primitive East" were launching a ferocious resistance against the Bolsheviks that was unparalleled in human history. Goebbels harbored a special bitterness for Churchill, whom he compared to Nero, a man who rejoiced as the last bastion of European culture went to its destruction.

Documents of both a personal and official nature from the last days in the Führer bunker are an important addition to this work, which will be eagerly read by both scholars and the general public.

JAY W. BAIRD
Miami University

KENNETH J. DILLON. *King and Estates in the Bohemian Lands, 1526–1564*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 57.) Brussels: Les Éditions de la Librairie Encyclopédique. 1976. Pp. xi, 206.

Following the debacle at Mohacs in 1526, Ferdinand I embarked upon one of the more remarkable reigns in the history of Bohemia. Unfamiliar with

the language, culture, religion, and parliamentary traditions of Bohemia and the incorporated territories of Moravia, Silesia, and Lusatia, the determined and calculating Ferdinand exploited the tensions among and within the Estates of the Bohemian lands and prepared the foundations of effective Habsburg rule. He began the process of the subjugation of Bohemia which was completed by his namesake and descendant, Ferdinand II, with the promulgation of the Renewed Ordinance of May 10, 1627.

This view of Ferdinand and his reign, presented by Kenneth J. Dillon, is not particularly new. In his study Dillon does, however, correct the tendency of many to view the relationship between Ferdinand and the Estates of the lands of the Bohemian Crown as one of unallayed hostility; after examining four key problems which affected the relationship between Ferdinand and the Estates—the Turkish wars, finances, religion, and the question of internal political control—he concludes that “at the core of their relationship were to be found both mutual reliance and mutual distrust” (p. 182). While neither particularly enjoyed it, king and Estates were forced to cooperate in all four areas.

As the wealthiest of Ferdinand’s territories, Bohemia bore the brunt of financing the Turkish wars and of relieving Ferdinand’s endemic impecuniosity. Nevertheless, the Estates did not take advantage of this situation to strengthen their own powers and privileges. Rather, lacking unity of purpose and will, the Estates of the various lands frequently worked at cross-purposes and, as Dillon observes, “each of the Estates was perfectly willing to support the king as long as this support entailed a diminution of the powers of another Estate” (p. 185). What was true for the relationship between the Estates of Bohemia and those in the incorporated lands was also true for the relationship between the various houses of each Estate. The conflicting self-interest of each segment of society provided Ferdinand with repeated opportunities to restrict the liberties and privileges which his subjects had been granted or had assumed during the tumult of the fifteenth century and to place his own supporters in key administrative positions.

But although the Estates of the Bohemian lands were their own worst enemies, Ferdinand was unable to effect the total subjugation of Bohemia. The Turkish wars and the problems in his other lands deflected much of his attention from Bohemia, while his policy of divide and rule required him to temper his actions in order to avoid measures which might have enabled the Estates to overcome their mutual hostility and to unite in opposition to Ferdinand.

Based on his 1973 Cornell doctoral dissertation,

Dillon’s book provides the English-speaking reader with an informative introduction to the parliamentary institutions and problems of the Bohemian lands and to the controversies and rivalries which enabled Ferdinand to increase royal authority and prerogatives. He has made effective use of the standard works, listed in the annotated bibliography, and of his own archival research. While his presentation of the religious situation is somewhat superficial, his analysis of the *berně*, or land tax, and its administration is most informative. Adequately but not extensively footnoted, this book is a welcome addition to the literature on Ferdinand and Bohemia and will be especially helpful to those seeking a clearly organized, well-written presentation of the complexities of Bohemian history during the first half of the sixteenth century.

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JEAN BÉRENGER. *Finances et absolutisme autrichien dans la seconde moitié du XVII^e siècle*. (Université de Paris, Sorbonne Series, number 1; Centre de Recherches sur la Civilisation de l’Europe Moderne, number 17.) Paris: Sorbonne Imprimerie Nationale. 1975. Pp. xx, 527. 138 fr.

The aim of this book is to create a “total history” of the government of the Austrian monarchy at a time when absolutism was entrenched in France and just before the Habsburgs were forced to relinquish Spain and to concentrate on their Danubian holdings. Though Jean Bérenger’s primary focus is state finances, which, he believes, lie at the heart of the relationship between government and governed, a continuing theme of his work also is the nature and limits of absolutism in Central Europe.

Using mainly state financial records in the Hofkammerarchiv in Vienna, Bérenger builds a tight, well-structured argument. As groundwork, he discusses the governmental organization of the monarchy, the problems of internal and foreign policy, and the state of the economy. Then he examines the finances themselves. Fixed sources of revenue, he writes, were few; and appropriations by the Diets of the various Habsburg lands, the only sources of income on a large scale, were notoriously uncertain. Even when the Diets approved contributions, court and military expenditures still regularly outstripped the appropriated funds. And the remedies—loans from Jewish financiers, subsidies from the Spanish relatives, financial juggling, even structural administrative reform—did not suffice.

For the main source of the monarchy’s problems

lay in the power of the aristocracy. Established on their huge estates, with a monopoly of power on the local level, the nobles naturally controlled the Diets that granted the appropriations. And because a few of the wealthy families also monopolized collegial offices in the imperial government, opposition to needed reforms came both from the bottom and from the top. Consequently absolutism, though an object of much theorizing at the Habsburg court, remained only a hope. The family's lands, in fact, were ruled by a diarchy, in which the aristocracy was a particularly obstreperous partner.

Béranger's book is an important addition to scholarly work on the Habsburgs. Though several narrower studies of Habsburg state finance exist, none attempts the integration of all aspects of government and the economy. Similarly, much work, especially in Hungary and in Czechoslovakia, has been done on the growth of great estates and the revival of serfdom; yet is primarily within an economic context. Hence Béranger's attempt to trace, in concrete terms, the political implications of the "second serfdom" is extremely valuable.

The organization of the study is based on a detailed outline, which is included as the book's table of contents. Though admittedly an aid in seeing the overall direction of Béranger's argument, such visible scaffolding produces annoying stiffness and a very choppy text—segments average about a page in length. Another problem perhaps is minor, but it leaves me uneasy. Virtually every one of Béranger's citations in the Czech language (e.g., pp. 175–76, 183, 306, 508, 516–18) contains errors in spelling, capitalization, and use of diacritics. Since the proofreading of other languages in the book meets a higher standard, it is difficult not to suspect basic unfamiliarity with those Czech works.

LINDA LONGFELLOW BLODGETT
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DEREK MCKAY. *Prince Eugene of Savoy*. (Men in Office.) New York: Thames and Hudson. 1977. Pp. 288. \$16.95.

The army was one of the most important, perhaps the most important single institution in the multinational and decentralized empire of the Habsburgs. The rise and decline in the fortunes of the dynasty were mirrored with a high degree of accuracy in the state of its military establishment. Among Austria's generals, Prince Eugene of Savoy, who loyally served three emperors, Leopold I, Joseph I, and Charles VI, long has occupied a pre-eminent position. His seventeen major victories raised Austria to the rank of a great European power. But, of course, Eugene was more than a

great field commander; he also was a military administrator, statesman, and a patron of the arts. Derek McKay, senior lecturer in international history at the London School of Economics, has undertaken to provide a biography covering all aspects of the prince's life, and he has succeeded.

To be sure, given the primacy of military affairs in Eugene's life, most of the book still deals with campaigns, battles, the policies of coalition warfare, and the struggle against an entrenched, and on occasion jealous, court bureaucracy. The author devotes roughly two-thirds of the volume to matters of this kind, and the remainder to a consideration of the man, the politician, and the statesman. He agrees that Eugene of Savoy was a great field commander, giving him high marks for his conduct of the campaigns against the Turks in Hungary and for his contributions in the War of the Spanish Succession. On the other hand, he is critical of Eugene as a military administrator. As McKay correctly points out, Eugene inherited an army fashioned by Count Raimondo Montecuccoli, made few changes, and was at his best amid the military and political stresses of war. As a reformer and administrator, he did not perform nearly as well.

Appointed president of the Hofkriegsrat in 1703, a position he held until 1736, Eugene accomplished relatively little. He sternly enforced the chain of command, created the basis for a more modern staff organization, and promoted technical education. He failed, however, to abolish the noxious practice of promotion by purchase or to root out corruption. Especially after 1718, the Austrian army deteriorated; and, if Eugene ever warned the Emperor Charles that political agreements were no substitute for a strong military force, "he contributed very little to effecting this himself" (p. 235). Moreover, despite the many paeans of praise heaped on Eugene as the protagonist of a truly multinational force, he was not free from prejudices. McKay notes that in particular he wished to exclude "Frenchmen, Italians, Swiss, Poles, Hungarians, and Croats" from the ranks, an attitude that endeared him to pan-German historians like Viktor Bibl, but did little to help the army become a cohesive and effective fighting instrument. Overall then, McKay does not agree with Napoleon, who considered Eugene one of the seven great generals of all times.

This mildly revisionist view, of course, follows recent findings of German and Austrian scholarship, most importantly the magisterial work of Max Braubach. The author has made excellent use of the latest scholarship, especially published works in English, German, and French, supplemented with research in the Austrian, Bavarian, and British archives, to document his account of

the political and personal aspects of his story. Unfortunately, perhaps, he did not consult the rich repositories in the Vienna Kriegsarchiv to give depth to his military accounts and instead has based his discussion of these matters on Alfred Arneth's three-volume biography and on the collaborative, twenty-volume campaign history, the *Feldzüge des Prinzen Eugen von Savoyen*, issued by the war archive. Together these do provide a mass of data, but they are now over eighty years old, and often the documents they cite no longer can be found under their original archival listings. A fresh look at the documents would have provided easier access and guidance for future research and even better support for the author's major points.

In balance, however, this is only a minor criticism. McKay provides us with a highly readable, well-constructed, and thoughtful narrative of the career of one of the key figures shaping early modern Europe. Specialists still will want to consult Braubach, but general historians as well as informed readers should consider this volume as the new standard biography in English.

GUNTHER E. ROTHENBERG
Purdue University

PETER FELDBAUER. *Stadtwachstum und Wohnungsnot: Determinanten unzureichender Wohnungsversorgung in Wien, 1848 bis 1914*. (Sozial- und Wirtschaftshistorische Studien, number 9.) Munich: R. Oldenbourg Verlag, 1977. Pp. 340. DM 49.

The history of Vienna during the reign of the Emperor Franz Joseph is more than the record of intellectual achievement so frequently stressed in recent studies on the Habsburg monarchy. It is also the record of unprecedented population growth and inadequate housing. In an important new publication Peter Feldbauer of the University of Vienna's Institut für Wirtschafts- und Sozialgeschichte directs attention away from the imposing cultural institutions of the Ringstrasse to the harsh realities that existed beyond the city in the so-called *Vororte*. Here, in suburbs that were autonomous until their annexation in 1890, workers sought refuge after 1848 from rapidly rising rents within the city limits. Although he is not the first to discuss the conditions encountered by the poor in their search for inexpensive housing, Feldbauer is not content, like many of his predecessors, primarily to describe. Instead he applies social and economic theory to analyze the question why housing conditions deteriorated for Vienna's lower classes between 1848 and 1914 and yet improved for the upper and middle classes.

Feldbauer's concluding chapter explains these contrasting trends. During periods of economic

growth, Vienna's housing industry found it difficult to compete for capital with more profitable areas of investment. This limited the supply of housing and raised rents for workers attracted to the city by the availability of jobs. Rents were also forced upwards by high mortgage rates and the high building costs of a labor-intensive construction industry that resisted modernization. In addition, rising land values in the Inner City and its environs favored the construction either of commercial buildings or expensive luxury apartments. Workers consequently were crowded into high-rent housing in the *Vororte*, where a combination of high prices for land, capital, and construction dictated the maximum utilization of building lots. Economic liberals, who placed their faith in the benevolent operations of a self-regulating market, blamed this situation upon administrative measures such as Vienna's building codes and its high tax on rental income. In contrast, Feldbauer contends that it was impossible for an essentially free market to adjust the supply of housing to demands generated by rapid population growth. Vienna's housing problem was, he concludes, the result of forces inherent in an economic system more concerned with profits than with social welfare.

Taken on its own terms, Feldbauer's book is a qualified success. Its conclusions are based upon a wealth of statistical information and are persuasive. The author's prose is, however, ponderous, and his tone is occasionally polemical. It is also unfortunate that Feldbauer has defined his topic so narrowly. The human consequences of crowded living conditions deserve more attention than he gives them, as do the effects upon residential patterns of inadequate public transportation and Vienna's regressive consumption tax. Finally, by deliberately reserving for a future study any discussion of the housing policies of the municipal and imperial governments, Feldbauer has missed an opportunity to write a much-needed comprehensive history of housing in nineteenth-century Vienna. It is to be hoped that when his second study is completed, he will address himself to the broader audience his topic and his conclusions deserve.

RONALD E. COONS
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DENYS HAY. *The Church in Italy in the Fifteenth Century*. (Birkbeck Lectures, 1971.) New York: Cambridge University Press, 1977. Pp. x, 184. \$16.95.

This volume originated in the Birkbeck Lectures at Cambridge for 1971. Denys Hay tells us that it was first his intention to survey the reactions of the Italian clergy to the Renaissance, but that he had

to give up this plan because of the absence of fundamental studies essential to its writing. His frustration became the touchstone for the present introduction to the history of the fifteenth-century Italian Church. In six brief chapters, he moves from historiography through ecclesiastical organization to the state of the clergy and the quality of religious life, ending with a chapter on the clergy and the Renaissance. The appendixes deal with Italian bishoprics, a list of fifteenth-century popes, promotions of cardinals, and Church archives in Italy.

Those interested in the history of medieval and Renaissance Italy—not just those concerned with Church history—will find this a useful book. Hay provides numerous *caveats*, for example regarding the uncritical use of important standard works of reference, that will alert beginners to problems. He deals with a great number of organizational questions that frequently present a maze even for the experienced scholar. Although his work is not intended as an introduction to current research on this topic, it does serve that function. There is, in fact, a great deal of good sense in this book and rather more information than its slim size would indicate.

The most disappointing section is that dealing with historiography. Hay is at home with the standard works and collections of sources, but his treatment of current research is spotty. To some degree, his discussion of special topics remedies this defect, but it does not entirely erase the impression he creates regarding the inadequacies of Italian scholarship. Though he pays tribute to the role of the *Rivista di Storia della Chiesa in Italia* in promoting “a sophisticated scholarship,” he does not accord merited recognition to the contributions of the pontifical universities in Rome, to the various historical societies and institutes, to *Sacro Cuore* in Milan, or to the excellent work being done under the auspices of various religious orders. I would certainly concur that much of the best of this work is recent, though that generalization does not pertain to the many excellent critical editions or to the preparation of useful tools such as manuscript catalogs. Nor does Hay spend much time on the significant contributions of foreign scholars, particularly those of the *École française* and the *Deutsches Institut* in Rome. Fortunately, his bibliography goes a long way in allowing his reader to fill in this picture. The study of Italian Church history has made considerable progress since World War II; Hay has presented a useful summary of its development with reference to the fifteenth century.

JAMES M. POWELL
Syracuse University

J. R. HALE. *Florence and the Medici: The Pattern of Control*. New York: Thames and Hudson. 1978. Pp. 208. \$14.95.

In several books, J. R. Hale has fully demonstrated his mastery of the art of historical synthesis, and his *Florence and the Medici* is by far the best general work on that subject in print. Based largely upon recent scholarship (Rubinstein, Kent, De Roover, Albertini, Cochrane) which has been discriminately exploited, the analysis is fresh and penetrating. It is neither hagiographical nor debunking in tone, but rather a thoughtful and sensitive attempt to explain the rise of the Medici and their complex relations with Florence. The style is characteristically evocative. What pleasure it is to read a sentence like this: “The security of the grand-ducal period has produced a view of it seen through eyes reddened by long and usually vain peering through keyholes” (p. 148).

The most fully developed chapters concern the rise of the Medici in the fifteenth century and the process by which they developed a “pattern of control” over Florence, with its deeply rooted republican institutions and its volatile populace. In explaining the ascent of the Medici, Hale pays appropriate attention to those features which have received particular emphasis in traditional historiography: their wealth, political acumen, patronage, personalities. He describes brilliantly the complex and elusive natures of Cosimo and Lorenzo the Magnificent. But Hale is also concerned with the Florentine context; he gives due weight to political developments, to economic trends, and particularly to the changes in the character and mood of the upper classes, which “help to explain why Florence ceased to be a republic” (p. 83). He describes the gradual and cautious adoption of control mechanisms by the early Medici, whose mastery of the government was less solid and secure than has sometimes been assumed. The chapters on the establishment of the principate tell a familiar story with clarity and economy, stressing again the gradual imposition of Medicean domination and the limits upon their authority. These chapters contain sharp, original perceptions on the complex symbiotic relationship between the Medici princes and their subjects, who were also (in varying degrees) partners and shareholders in the regime.

The later chapters on the principate are more sketchy, because the historiography is thinner, and because the city and her rulers “in the forgotten centuries” are (*pace* Cochrane) less interesting. Hale’s treatment of the reigns of the Medici grand dukes is more narrative and biographical than analytical, though he does not neglect the record of

demographic and economic decline and sagging power which forms the context of Tuscan history after the 1620s. The final chapter on the last miserable grand dukes, Cosimo III and Gian Gastone, contains a particularly interesting section on "the legendary afterlife of the Medici," where Hale reminds us that European interest in that family and its city did not become intense until the late eighteenth century.

GENE BRUCKER
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N. TODOROV. *Balkanskii gorod XV–XIX vekov: Sotsial'no-ekonomicheskoe i demograficheskoe razvitie* [The Balkan City from the Fifteenth to the Nineteenth Century: Socioeconomic and Demographic Development]. Translated by G. K. VENEDIKTOVA. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 515. 3 r. 90 k.

The book reviewed here is a Russian translation of the Bulgarian original published in Sofia in 1972. It is, without question, one of the outstanding contributions to our understanding of the socioeconomic and demographic history of the Ottoman Balkans.

This major work is divided into two parts. The first, entitled "The Place of the Balkan City within Ottoman Feudalism between the Fifteenth and Eighteenth Centuries," consists of three chapters, the first on the urban population of the Balkans in the fifteenth and sixteenth centuries, the second on Ottoman state regulation of the Balkan city, and the third on the development of economic inequalities within the urban population in the eighteenth century. Part two, "New Trends in the Development of the Balkan City," is an extensive, three-part study of the transition from feudalism to capitalism in the Ottoman Balkans at the end of the eighteenth and the first half of the nineteenth century, of the demographic structure of Serbian, Greek, and Bulgarian cities during the better part of the nineteenth century, and of the crafts and their income distribution in the Danubian *vilayet* of the empire (present-day northern Bulgaria) in the 1860s and 70s. Besides an introduction and conclusion, the book includes extensive notes, six appendixes, a list of archival sources, a glossary of terms prepared for the Russian edition by I. L. Fadeeva, an index of geographical names, and a series of illustrations. The Russian version also contains a foreword by V. D. Konobeev.

N. Todorov bases his book on a large number of primary and secondary sources. He is well acquainted with both Western and Soviet historiography of the Ottoman Balkans as well as with the very extensive contributions made, especially after World War II, by other Balkan historians, Greek

and Yugoslav above all. The main part of the book, however, rests on several series of Turkish judicial papers (*kadis' sicils*) of Ruse (1656–1870), Sofia (1604–19, 1870–79), and—most extensively—Vidin (1698–1846), supplemented with the *defters* from Babadag, Varna, Hadžioglu Pazardžik, Kustendža, Mačin, Medidia, Provadia, Silistra, Tulča, and Šumen. All of these sources are from the Oriental Section of the Bulgarian Historical Archives in the Public Library of St. Cyril and St. Methodius in Sofia. These are of greatest value, especially for the late seventeenth and early eighteenth centuries for which other archival sources are scarce. In addition the author has used account books and registers of the Ottoman guilds in about twenty cities, including Karlovo, Tatar Pazardžik, and Eski Dumaja for the nineteenth century. Finally, he has culled the rich archival deposits of the Giumrushgerdan firm in the Central State Historical Archives in Sofia. It should be pointed out that the above sources are most numerous for the eighteenth and nineteenth centuries, very scanty for the seventeenth, and nonexistent for the sixteenth. Todorov relies on H. Duda and G. Galabov's edition of *kadis' sicils* from Sofia (*Die Protokollbücher des Kadiamtes Sofia* [Munich, 1960]), to fill in the gap partially.

It is impossible to do justice to such an important work in the space provided. Most of the statistics and its underlying methodology, presented in forty-eight larger tables, are pioneering. This is especially true of the demographic breakdown and tax structure of Balkan cities. Very revealing is the statistical information on the religious and ethnic composition of Bosnia, Serbia, Bulgaria, and Greece. All in all, the book is an excellent source of precious data on the changing kaleidoscope of peoples, races, religions, and occupations of the Balkans.

I will have to confine my criticism to one solitary, but crucial, problem—the origins and emergence of capitalism in the half century or so between Selim III's *Nizam-i Jedid* and the *Tanzimat* (1790s–1840s). Todorov argues that the rising disparity within the city populace led to the accumulation of capital on the part of the upper strata of the subject peoples (Greeks, Bulgarians, and Serbs) and its investment in state-protected and state-oriented, monopoly enterprises, such as the textile factory of the Giumrushgerdians. Yet he admits that most so-called capitalists preferred to invest in traditional trading and manufacturing rather than in industrial complexes. It seems to me that the reluctance of so-called Balkan capitalists to build and invest in factories is a telling sign of the underdeveloped nature of Balkan capitalism as well as of Ottoman neglect of personal and property rights. There is still much that we do not

know about the emergence of capitalism in the Balkan core, but after Todorov the way has been cleared.

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JÓZEF CHLEBOWCZYK. *Procesy narodotwórcze we wschodniej Europie środkowej w dobie kapitalizmu (od schyłku XVIII do początków XX w.)* [Nation-Forming Processes in Eastern Central Europe in the Era of Capitalism (from the Close of the Eighteenth to the Beginning of the Twentieth Century)]. Warsaw: Państwowe Wydawnictwo Naukowe, and Śląski Instytut Naukowy, Katowice. 1975. Pp. 376. 88 Zł.

This is an ambitious and a difficult book. Reviewers in Poland have widely hailed it as one of the most important achievements of postwar Polish historical writing. Józef Chlebowczyk's aim is to develop a model of the processes of nation-building among the subject (minority) peoples living in the dynastic-territorial states of nineteenth-century East Central Europe—the Habsburg monarchy, the European portions of the Ottoman empire, the western lands of tsarist Russia, and the eastern provinces of Prussia. Chlebowczyk distinguishes between a Western European pattern of nation-building, where the embryonic nation-states predated and molded the national language and consciousness, and the more complicated situation in East Central Europe, where the presence of multinational states caused a radically different course of national development. There, among the manifold ethnic-linguistic communities, "the principal factor in the nation-forming process was not the state, but language" (p. 335).

Chlebowczyk delineates schematically the several stages of national development. In the initial phase, the local native dialects coalesced into a modern literary language, which permitted the minority to counter assimilationist pressures. In the next phase, as the consciousness of belonging to a distinct ethno-linguistic community grew, the national awakeners pressed demands for linguistic equality with the dominant language and culture (German, Magyar, or, in the case of Galicia, Polish, etc.). A national ideology emerged, which became identified with a specific territory. These developments led to demands to institutionalize the "national existence" in the form of autonomy, accompanied by a growing tendency to call for full sovereignty. This process inevitably led to clashes between the aspiring subject nationality and the dominant culture and government of the state

within whose boundaries the minority group found itself.

The distinction between Western and Eastern European patterns of national development is certainly not original; rather, it is Chlebowczyk's postulation of a schematic model applicable throughout the nationally diverse reaches of East Central Europe (as well as for Alsace-Lorraine, the German-Danish borderlands, and the Caucasus) that should especially command the attention of students of nationalism. Yet it is precisely Chlebowczyk's claim that his model is broadly applicable, with only slight exception (e.g., he eschews discussion of Jewish nationalism in Eastern Europe), that opens his argument to question.

Specialists in the history of one or another of the nations of East Central Europe will find instances where Chlebowczyk's model does not apply neatly to their subject. A native of Cieszyn Silesia, Chlebowczyk is at his surest in analyzing the factors that determined national development in this German-Czech-Polish borderland region. Most of the examples he uses to buttress his thesis are taken from the Habsburg monarchy, especially Cisleithania. This reliance is also reflected in his source basis, which draws primarily on the German, Polish, Slovak, and especially Czech literature on nationalism.

A Marxist historian, Chlebowczyk nonetheless has little use for the "uncritical" postwar Soviet authors I. I. Udaltsov, S. B. Kan, and R. A. Averbukh whose writings on the national question in the Austrian monarchy have tended to "canonize" the utterances of Marx and Engels and minimize or skip over inconvenient issues. But unfortunately Chlebowczyk would appear not to have taken advantage of the works of a growing number of sophisticated present-day Hungarian Marxist historians, many of whose works on the national question in Transleithania are available in the principal world languages.

Chlebowczyk's book is difficult not only in subject, but also because of the author's ponderous style, his choice of a topical rather than a chronological approach, and his elaborate schematic model. Regrettably, this provocative work received an inordinately small, single printing, which sold out within several weeks.

LAWRENCE D. ORTON
Oakland University

JERZY SKOWRONEK. *Polityka bałkańska hotelu Lambert (1833–1856)* [The Balkan Politics of the Hotel Lambert (1833–1856)]. Warsaw: Wydawnictwa Uniwersytetu Warszawskiego. 1976. Pp. 278. 26 Zł.

In recent years Polish historians have returned to the study of mid-nineteenth century Polish con-

servative groups and movements. Several of these works discuss Prince Adam Czartoryski and the Hotel Lambert's political initiatives throughout Europe. In this volume Jerzy Skowronek attempts to deal with the extremely complex involvement of the Hotel Lambert in the Balkans. Though not the first to treat this question, Skowronek is the first to publish any work concerned exclusively with the Balkan aspects of Czartoryski's diplomacy.

Basing his work on archival sources, Skowronek approaches the problem within the context of the development of Balkan nationalism. His major thesis is that the Hotel Lambert played a leading role in the growth of Balkan nationalisms through 1.) its direct involvement with various national groups and 2.) its diplomatic support of these causes at the Ottoman Porte as well as in London and Paris. This has been a long-recognized aspect of the Hotel Lambert's activities in the Balkans, previously studied by both Marcell Handelman and Marian Kukiel. Skowronek, however, contends that earlier studies have concentrated too exclusively on the concept that these initiatives were in the sphere of "classical foreign politics or diplomacy." He stresses, rather, the view that these activities rarely were so exclusive. Though necessity required the Hotel Lambert to use the forms of classical diplomacy, it "acted jointly with particular individuals or with entire groups within the bosom of national movements in the Balkans. It very frequently undertook initiatives relating to the sphere of the internal life of the particular countries (cooperation in matters of internal reform, education, cultural development, or the religious question)."

Skowronek's approach to the Hotel Lambert's actions is chronological. Within this framework he attempts to weave several threads simultaneously: the growth and decline of the Hotel Lambert, its direct role in Balkan affairs, and the changing emphasis of its broader political activities. Skowronek begins with a solid discussion of the origins, structure, and organization of the Hotel Lambert. He then traces its growing diplomatic sophistication and increasing political influence, as it modified its early, highly idealistic, and naive visions of enlisting the Ottomans to fight Russia in the cause of Polish freedom. By the late 1830s and early 1840s it focused its activities on the creation of a unified Balkan Slavic state which would remain under Ottoman control. By the mid-1840s Czartoryski's organization attained its greatest influence as it played a key role in the development of the Serbian *Načertanije*, attempted to work with Ljudevit Gaj's Illyrian movement, and enmeshed itself in religio-nationalistic activities in Bosnia and Bulgaria. From this point, the Hotel Lambert rapidly lost influence as Balkan nationalisms and Polish aims no longer coincided. Despite a brief

resurgence in 1848-49 the Hotel Lambert's decline, marked by the loss of key members, an aging leadership, and the withdrawal of British and French support, continued unabated.

Skowronek overlays this narrative with a discussion of the organization's political aims. These, modified over time, were: 1.) to develop an independent center for a Balkan Slavic state, preferably within the Ottoman structure; 2.) to encourage the growth of a strong anti-Russian and, later, anti-Habsburg orientation in this area; 3.) to establish a Catholic or Uniate religious orientation among the Balkan peoples (a goal quickly abandoned); and 4.) to use these other aims as political devices to create an independent Poland. The result of Skowronek's work is a complex story. He does an excellent job of keeping all these trends in perspective, leading the reader through the maze of politics and diplomacy affecting the Balkans, and outlining the Hotel Lambert's role in the growth of Balkan nationalisms. Although his chronological approach can confuse a reader lacking familiarity with Balkan history—simply because of the numerous activities pursued concurrently by the Hotel Lambert—on the whole Skowronek has produced a work which should be read by all scholars of modern Balkan and Polish history.

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S. VICTOR PAPACOSMA. *The Military in Greek Politics: The 1909 Coup d'État*. Kent, Ohio: Kent State University Press. 1978. Pp. xi, 254. \$12.50.

On August 28, 1909, the military garrisons of Athens successfully staged a bloodless coup d'état. The recently organized Military League, under whose leadership this demonstration of strength was carried out, aimed at forcing the government to embark on an extensive reform program primarily designed to facilitate the reorganization and buildup of the armed forces. This incident was symptomatic of a widespread discontent with the government and the dynasty, aggravated by persistent economic stagnation and by the government's failure to advance Greece's nationalistic aspirations. The military revolt, its background, and immediate aftermath form the subject of this monograph.

The Military League did not, through its intervention, question the established sociopolitical structure, nor did it contemplate the kind of reforms that could lead to a radical social transformation. The military revolt, as the author correctly concludes, was no more than a reformist coup d'état. Few of the officers involved displayed radical tendencies. On the whole, their stance did not transcend the radical populism characteristic

of the political ferment of the period. The military evinced little interest in long-festered social ills, and most of the reforms enacted under the league's pressure were of little social significance.

Of greater importance were the numerous reforms enacted following the league's withdrawal from active politics in March 1910, especially those formulated under Eleftherios Venizelos' guidance. Herein lies the historical significance of the military revolt. For it was this incident which paved the way for Venizelos' rise, ushering in a period of important reforms that contributed to the gradual *embourgeoisement* of the Greek state, and to the preparation of the country for the Balkan Wars of 1912-13. Still, the traditional social and political structure was not fundamentally affected by the rise of the new political forces. After a brief period of confusion and disorientation the conservative and reactionary elements were able to regain some of their lost ground. The growing polarization of the political forces that loomed ominously after 1912 must be seen as one of the consequences of the events of 1909-10.

This is a carefully researched study based on all available published and unpublished Greek sources, as well as on relevant materials of the British, German, and Austrian diplomatic archives. The author addresses himself primarily to the political developments of the period. The league's active involvement is thoroughly discussed, but the period between the military's withdrawal from active politics in March 1910 and December 1913 is somewhat slighted. The inclusion of the epilogue, in which the author attempts to draw comparisons between the 1909 revolt and subsequent military interventions, is unfortunate, because its weakness does no justice to the book. In general, the book is less strong on analysis than on the exposition of political developments. It remains, however, the best study in any language of the political developments of the period. For a broader social analysis, Papacosma's book should be read together with George Deretiles' recently published study *Koinonikos metaschematismos kai stratiotike epemevase 1880-1909*.

GEORGE B. LEON

Memphis State University

ALEXANDRU DUȚU. *Romanian Humanists and European Culture: A Contribution to Comparative Cultural History*. (Bibliotheca Historica Romaniae, number 55.) Bucharest: Editura Academiei Republicii Socialiste România, 1977. Pp. 196. 10 L.

The author of this book is a well-known Romanian academician, a member of the South-East European Institute in Bucharest, and at the same time

the adjunct editor-in-chief of the professional publication *Revue des Études Sud-Est Européennes*. The distinguished author of numerous published articles and learned papers presented in the programs of international congresses abroad, Alexandru Duțu has published several works related to the subject of this book, which appeared in Romanian in 1974.

Humanism is a term that has acquired many meanings, and its use has engendered considerable confusion. Duțu seems to share the point of view of various writers of the twentieth century who make humanism "a suitable term to characterize any view of the world for which humanity is the central object of interest," and he concentrates on the problem of how Romanian humanism "fits in the patrimony of world culture" (p. 19). The problems of influences and assimilations in Romanian culture were raised by the author earlier in his book *Sinteza și originalitate în cultura Română, 1750-1848* (Synthesis and Originality in Romanian Culture, 1750-1848 [Bucharest, 1972]), where, in the process of outlining "cultural models" during periods of cultural intensity, he was able to identify three phases: humanism of society, Renaissance patriotism, and the Romanticism of 1848.

In his present book the author returns to the theoretical aspects of the problem and takes up again the question of the emergence of social Romanian humanism within the framework of European culture of the seventeenth and early eighteenth centuries. In the first chapter of this book, "Patrimony of World Culture and the Diversity of Culture," we are confronted with a system of analysis which might help in the classification of European cultures within a coherent totality: an interdisciplinary approach, with cultures placed in comparison as fragments in a totality, the analysis of disturbing and stabilizing factors that have agitated the cultural life of European society in modern times, as well as the study of accounts that portray the similarities and the differences of cultural zones expressed in a rhythm of cultural contacts. All contribute to a better understanding of the period of European humanism. Within the framework of this period Romanian humanism, in full development toward the end of the seventeenth century, became a national component of a European trend.

The second chapter, "Approach and Distance," deals with a comparison of the cultural program of Romanian society with the programs of the West—starting with mental structures which emerge from an analysis of figurative language, oral culture, and a series of books (in a section entitled "The Word, the Book and the Image"). It contrasts the image of European civilization formed in Western writings with the ones observed in the writings of Romanian humanists. In the last

chapter, "Connexions and Reconsiderations," the author discusses the place of Romanian culture among the interdependent cultures of this epoch, such as Holland and Switzerland, and identifies a new universality emerging from Europe and formed in this cultural center. Duțu also points out that "the Romanian humanists were obviously attracted by Western Europe but remained faithful to Byzantine universality . . . information taken from the West had been synthesized with oriental elements and autochthonous ones . . ." (p. 31). Recognizing the relations with neighboring cultures, the author maintains that the Romanian humanists, often separated by feudal and artificial boundaries, nevertheless managed to develop "the idea of the oneness and universality of human civilization" (p. 32).

Duțu presents his information in the light of early and modern literature. The book lacks a relevant bibliography, but is carefully footnoted and has a useful index. Otherwise, this is a very interesting book presenting an interdisciplinary approach combining the investigation of literary works with the analysis of other source materials so that we may grasp both the thoughts and feelings of leading cultural figures and realize the great wealth of cultural accomplishment available to all European societies.

DEMETRIUS DVOICHENKO-MARKOV
Monmouth College

ANDREI OȚETEĂ. *Pătrunderea comerțului românesc în circuitul internațional (în perioada de trecere de la feudalism la capitalism)* [The Penetration of Romanian Trade into International Commerce (in the Age of Transition from Feudalism to Capitalism)]. ("Biblioteca Istorică," number 48.) Bucharest: Editura Academiei Republicii Socialiste România. 1977. Pp. 168. 11 L.

Despite the concentration on social and economic history by Romanian historians since the end of the Second World War, a concentration which has resulted in the systematic publication of collections of sources and the elaboration of new methodologies, there has been no general history of Romanian commerce. One may cite the pioneering works of Nicolae Iorga on the history of Romanian and Near Eastern commerce (1915-25) and of Gheorghe Zane on money and credit in the Romanian principalities (1930); but until the appearance of the volume under review here no comparable synthesis was available that put to use the substantial accomplishments of Romanian historiography in the last twenty years.

In this his last published work, the late Andrei

Oțetea has given us an overview of the research and writing done in the past century on the history of Romanian commerce. He believed that a true synthesis was still many years away because much basic research remained to be done on such problems as the history of prices and the accumulation of capital. Nonetheless, his own work provides a comprehensive view of the state of the art for the crucial transition period between the Treaty of Kuchuk Kainarji in 1774 and the Treaty of Adrianople in 1829. Oțetea has assembled an exhaustive bibliography of published primary and secondary materials and, for the first time, has made use of unpublished Russian consular reports from the Archives of the Ministry of Foreign Affairs in Moscow for the latter part of his period. He has been careful to use documents that relate to the operations of commerce, since they reflect its actual state more accurately than governmental laws and regulations, which often indicate merely what the state of things should be.

Oțetea saw many signs of a quickening of economic activity in Moldavia and Wallachia: the introduction of Romanian agricultural products on the international market; an increase in the exchange of goods at home; a rise in the amount of money in circulation; and numerous, though often unsuccessful, attempts to found manufacturing enterprises. His account of the economic penetration of the principalities by the great powers is particularly valuable. Austria took the lead in the 1780s and quickly assumed a position second only to that of the Ottoman Empire as a Romanian trading partner. Oțetea described in detail the "integration" of Moldavia and Wallachia into the European trading system through a growth of exports and imports and a flourishing transit commerce, pointing out how foreign trade, in turn, promoted the growth of cities as business centers, led to an increase in the number of markets and fairs, and encouraged the development of a numerous merchant class. Yet, despite these encouraging signs of capitalist development, the economy of the Romanian principalities did not achieve its full potential. The impediments in the way of commerce and of economic growth in general were numerous and varied, but, as Oțetea suggested, they were all related directly or indirectly to the Ottoman commercial monopoly and political suzerainty. For example, the Ottoman government fixed prices and imposed quotas and allowed foreign merchants a privileged status, all of which discouraged local initiative; and the "chaotic" Ottoman monetary system and the deplorable condition of transport facilities made commerce risky. There were also serious local impediments: the varieties and arbitrariness of taxation, manifold administrative abuses, and the economic privileges

of the boiers, who stubbornly resisted measures that would allow commerce to develop in accordance with its own interests.

There emerges from this well-documented discussion of all the above matters both a broad view of the Romanian economy of the period not readily available elsewhere and a stimulus to future research. Historians of Romanian and Southeast European social and economic development will feel keenly the absence of Andrei Oitea's scholarly example.

KEITH HITCHINS
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PAUL D. QUINLAN. *Clash over Romania: British and American Policies toward Romania, 1938-1947*. (American Romanian Academy of Arts and Sciences, number 2.) Oakland, Calif.: The Academy. 1977. Pp. 173. \$10.00.

Thanks to newly opened British and American records, historians of diplomacy are eagerly conducting forays into the mass of primary material that may enable them to write or rewrite the story of the decisions that shaped the fate of nations in the critical years surrounding World War II. Paul D. Quinlan, using this material as well as the published documents, has chosen Romania as the focus of his explorations. In wending his way from the Munich crisis to the firm establishment of Communist rule in Romania by the end of 1945, he has a good eye for the more significant issues, uses his material judiciously, and for the most part avoids entrapment in the controversies over what the West did or did not do, should or should not have done, to save Eastern Europe from the Russians or to save the world from the Cold War.

Faithful as it is to the evidence, however, the book suffers from a narrowness of approach, making little effort to relate decisions about Romania to a wider context. For example, the story of the German "ultimatum" to Romania in March 1939 and Tilea's urgent appeals for British action is told here in interesting detail—though Tilea's memoirs, unfinished and unpublished at the time of his recent death, could tell us more—but for lack of background it seems to attribute Britain's turn from a policy of appeasement to one of building anti-German alignments in Eastern Europe largely to events in Romania rather than in Czechoslovakia and Poland. Similarly, the treatment of the British and American roles in the armistice negotiations and in occupied Romania needs the broader perspective of the war and of the development of a strategy for dealing with Moscow in postwar Europe.

The main revelation of the book, as the author presents it, is "the part played by the United States in the attempt to overthrow the Groza government in August 1945," an "abortive coup" that "has remained almost totally unknown to historians." The episode in question followed the decision taken by the Big Three at Potsdam to begin consideration of peace treaties with Romania and other countries, and a subsequent statement by the Western Allies that they would sign peace treaties only with "recognized democratic governments." The main question is whether the United States, on the basis of that decision, adopted a new policy in Romania going beyond nonrecognition of the Communist-dominated Groza government (imposed by the Soviets the previous March) to the point of working with anti-Communist forces in Romania to overthrow it. The State Department's instructions to the acting U.S. political representative in Bucharest, Roy Melbourne, were that he could tell King Michael and the opposition leaders that the United States did not regard the existing government as meeting the criteria of the tripartite decision and looked forward to the establishment by the Romanians themselves of a representative democratic government. Perhaps these instructions were ill advised in view of the known intentions of those leaders, but they were not a plan or a commitment to do anything. When the king forced the issue by asking Groza for his resignation and the Allies for assistance, he got neither. The entire affair, though termed risky by the British who did not support it, was more in the nature of a clarification of a political position, of which the Russians were also informed, than of a planned coup d'état. The Romanians may have been deceived, but unless it can be shown that Melbourne exceeded his instructions—and the fact is that the State Department approved his actions—the conclusion must be that there was no American promise or intention to back up, other than with words, a move to overthrow a government created and supported by the Soviet Union in a former enemy state occupied by the Soviet army. By the end of the year, as the author reminds us in his account of the Moscow conferences of December 1945, even the brave words had been compromised.

JOHN C. CAMPBELL
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RANDOLPH L. BRAHAM. *The Hungarian Labor Service System, 1939-1945*. (East European Monographs, number 31.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. x, 159. \$11.00

Of the approximately 650,000 Jews living in Hungary in 1939, some 70 percent were to perish in the course of World War II. Most Hungarian Jews lost their lives in 1944 and 1945 in the German death camps. But already in the first years of the war the Jews of Hungary had had to contend with a Hungarian institution, the military labor service, that inflicted on them great indignities and sadistic cruelties. Indeed, many thousands of Jews had died before Adolf Eichmann ever set foot in Hungary. This unique system, in which Jews and other political "unreliables" were employed in various wartime projects, is the subject of Randolph L. Braham's compact study.

Braham is an indefatigable student of the Holocaust in Hungary. The many years of studying and editing the relevant documents are now bearing their fruit, for Braham's general study of the fate of Hungary's Jews will apparently appear in the near future. The volume under review, however, focuses on only one aspect of that tragic story. The Hungarian labor service system was designed as one solution to the "Jewish problem" in Hungary. Able-bodied Jews were deemed too unreliable to be allowed to bear arms as soldiers in the regular Hungarian army. Instead, they were organized into military formations under the command of Hungarian army officers and assigned to various hard labor projects of military importance. During the course of the war, labor service companies were employed not only in Hungary but also on the eastern front in the Ukraine and in the Bor copper mines of Yugoslavia. It was particularly in their assignments outside Hungary that the Jews of the labor service were subjected to most violent and inhumane treatment. In many cases sadistic Hungarian officers, ignoring instructions from their superiors in Budapest, enacted their vulgar, often murderous fantasies. In addition to countless Jews who could not endure the exhausting work and physical abuse, many died in massacres carried out by German or Hungarian officers. Though some Hungarian officers were appalled by the excesses they witnessed, their efforts to protect the Jews were for the most part futile.

The strength of Braham's study lies in the thoroughness and detachment with which he describes a complex, emotional, and little-known story. The workmanlike narrative is complemented by the poignant personal accounts of survivors that are printed in the appendixes. What is most lacking in this study is an analysis of some of the important issues that the author directly or indirectly raises. For example, one wonders why some Hungarian military officers were brutally anti-Semitic, while others were motivated by humanitarian principles. Was it perhaps social origin or religious affiliation that made the difference? It is to be hoped that in

his forthcoming book on the Holocaust in Hungary, Braham will offer his thoughts and conclusions on broader issues of this kind.

THOMAS L. SAKMYSTER
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TERESA ZIELIŃSKA. *Magnateria polska epoki saskiej: Funkcje urzędów i królewskiej w procesie przeobrażeń warstwy społecznej* [Polish Magnates under the House of Wettin: The Functions of Public Offices and Crown Lands in the Process of the Transformation of Social Classes]. Summary in English. Wrocław: Ossolineum. 1977. Pp. 235. 60 Zł.

This is an excellent addition to the expanding literature on the magnates and the structure of politics in pre-Partition Poland, subjects that recently have been attracting attention in Poland and abroad. Zielińska does not aspire to a broad study of eighteenth-century magnate life. Instead, using genealogies and an impressive range of archival sources, she describes the mechanics by which families advanced into the magnate class, consolidated their position, and, occasionally, fell back into the ranks of the "mere gentry." The author generally confines herself to the period of the Saxon kings (1697–1763), sometimes also touching on Sobieski's reign (1674–96). Regrettably, Lithuanian politics are excluded, so that little is said of the rise of the Czartoryskis—one of the most important aspects of the eighteenth-century Polish-Lithuanian Commonwealth's history.

Like most recent investigators, Zielińska defines the magnate class simply as the group of *szlachta* who held high offices (*wojewoda*, the "greater castellans," royal ministers) carrying ex officio membership in the Senate, the upper chamber of the Sejm. Polish kings granted no titles (though imperial titles were recognized), and the only formal gradation within the *szlachta* was the hierarchy of senatorial seats prescribed in 1569. Competition for these places, however, was intense, and in making appointments the king exercised one of his few opportunities for independent maneuver. Nominally responsible for maintaining order within their bailiwicks, *wojewodas* and castellans in fact held their offices for profit, supplementing their modest salaries with rake-offs from local courts and by brokering in the lower offices eagerly sought by the gentry. The magnates' greatest source of profit, however, was the possession of royal domain.

The terms by which the king distributed royal domain to *szlachta* tenants were established in the 1560s, climaxing the gentry campaign against magnate preponderance known as the Execution of the Laws movement. Domains were to be held

for life but not in perpetuity, and 20 percent of the revenues were to go to the government. The domain-holder could usually contrive to retain the rest. This system prevailed throughout the Saxon period, and Zielińska devotes considerable space to analyzing it. Not surprisingly, she finds that the more lucrative the domains, the higher ranking the senatorial tenants. Grants of domain to successful *szlachta* politicians largely explain the rise of most leading Saxon-era magnate lines, notably the Lubomirskis, Potockis, and Zamoyskis. Domain-holders' profit margins permitted the purchase of extensive private property—which, however, was subject to division among heirs. Entails (*ordynacje*), which for example assured the Zamoyskis' survival despite political difficulties, were uncommon since they could be established only with the Sejm's consent. Normally, Zielińska demonstrates, a magnate family's survival required not only judicious marriage alliances but also continued royal favor in distributing domains.

Zielińska has established some important facts about the Saxon-era magnates. Notwithstanding old senatorial families' possession of the most prestigious offices and the most desirable domains, there was a rapid influx into the magnate class of "new men": over a third of the senators were of nonsenatorial background, usually recruited from the gentry or from Saxony. Polish resentment at the Saxon kings' promotion of foreigners becomes more understandable, and the ephemeral reign of Stanisław Leszczyński (1706–09) stands out as witnessing an attempted entrenchment by old senatorial elites.

The workings of royal patronage are not investigated here; perhaps such a study would reveal that Polish kings exercised unexpected influence in making and unmaking magnate dynasties. When this Namieresque study is undertaken, however, Zielińska's book ought to prove a valuable starting point.

JAMES MILLER
Stockton State College

JACEK SOB CZAK. *Wielkopolskie sądy ziemiańskie* [The District Courts of Great Poland]. (Poznańskie towarzystwo przyjaciół nauk, Prace komisji historycznej, volume 30.) Summary in French. Warsaw: PWN. 1977. Pp. 95. 22 Zł.

This is a valuable contribution to our knowledge of the Polish system of justice in the eighteenth century. The scope of the book, however, is limited to 1.) only one province, Great Poland; 2.) only one class, the gentry; 3.) only lower, district courts; and 4.) only a period of a few months during which this particular court was acting. In spite of, or

rather thanks to, this limitation, this book of only ninety-six pages maintains the quality of a scholarly study.

The new reformed district courts were established in February 1792 as an integral part of the great political reforms of the Constitution of the Third May, 1791. The tradition of gentry district courts goes back as far as the fifteenth century. The privilege of 1422 declared that the district judge was to be appointed by the king for life on the recommendation of the whole body of local gentry from among their number. In 1578 two gentry supreme courts, the so-called tribunals, were established, one for Poland and one for Lithuania. This tribunal in Poland was the final court of appeal for civil cases that involved nobility.

In the eighteenth century the old system of justice ceased to perform its function. In particular the district courts met less and less frequently because factional political fights hindered the appointment of judges. As a result the number of cases directed to the supreme courts—the tribunals—steadily increased, and there was a serious delay in meting out justice. Attempts at reforms were made—among others both G. B. Mably and J.-J. Rousseau published treatises on the subject—but it was the commission of the so-called Great Diet (1788–91) which finally worked out the project of the reforms.

Because the structure of the new courts was not uniform for the whole country, the case of Great Poland is not relevant for the rest of the Commonwealth. The new courts worked only several months. In September 1792 they were abolished together with the whole Constitution of the Third May, 1791 by the intervention of Catherine II of Russia.

Had the author also discussed the court proceedings this would have been a very interesting book, but Jacek Sobczak limits himself to describing the organization and functions of the reformed district courts of Great Poland. These were serious and dull, but useful and necessary, matters—as is the book by Sobczak.

ANNA KAMINSKA
University of Oregon

I. S. MILLER *et al.*, editors. *Ocherki revoliutsionnykh svyazei narodov Rossii i Polshi, 1815–1917* [Essays on the Revolutionary Bonds between the Peoples of Russia and Poland, 1815–1917]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 601. 3 r. 50 k.

A collective effort by Soviet historians has produced this survey of Russian-Polish revolutionary connections from the inclusion of Poland under tsarist rule, as the Congress Kingdom, until the

October Revolution. This century-long period is broken into stages of revolutionary action, based on the social force which spearheaded the struggle at a given time, that is, the gentry, the radical intelligentsia, and the Marxist proletariat.

The authors' declared intention is to point out what was uniting rather than dividing the revolutionaries of the two nations, and they scrupulously list instances of personal or organizational contacts, of cooperation, and mutual good will. Nevertheless, the text demonstrates that this relationship had its share of strain, which reflected mistrust and prejudices as much as differing levels of political development. The dashed hopes of the Polish insurgents of 1863 for a revolution by their Russian sympathizers left a bitter aftertaste. Also, there loomed always the intractable issue of the borderlands—Lithuania, Belorussia, and the Ukraine—which neither side was willing to write off from its own vision of the future.

The presentation of the topic is structured like a pyramid, broad at the bottom, then narrowing sharply. Until the moment marked as the watershed—the emergence of Marxist organizations—the book deals with the links between all the revolutionaries of Poland and Russia. In its second half, covering the quarter century before 1917, the focus is on the relations of the Russian Social Democratic Workers' Party with its ally, the Social Democracy of the Kingdom of Poland and Lithuania (SDKPiL), and tangentially, with the Polish Socialist Party (PPS). The latter is described as nationalist and reformist with the addition of the nebulous term "petit bourgeois." The authors' view of this party is harsh, more so than that of contemporary historians in Poland. By contrast, there is a profusion of praise for the SDKPiL, except for the criticism that its rejection of Poland's right to national self-determination was excessive, and had to be condemned by Lenin. Yet, it appears from the book that, aside from the largely semantic dispute over the national question, there were more fundamental differences between the Russian Social Democrats and their Polish confederates. The SDKPiL remained wary of the Leninist principle of centralism, stood for intraparty democracy, and occasionally took the side of the Mensheviks. Conversely, when in the years 1909–12 the SDKPiL was rocked by strife, the Bolsheviks supported the rebellion against its leadership.

The book, with its chapters written by several authors, is not free from flaws, structural and otherwise. Its composition suffers from the imbalance between the first and second parts, and its disregard for anything unrelated to the SDKPiL and Russian Social Democrats belies its title. All too often the narrative turns into the cataloguing of events and examples. Clearly, the abundance of

information does not result in a synthesis. Despite its distorted presentation of the subject, the book is well researched and cites new sources from Soviet and Polish archives. The facts upon which the authors would rather not dwell are usually mentioned briefly instead of being left out. Thus, a casual remark acknowledges extensive cooperation by the PPS with Russian Socialist Revolutionaries. Still, there are outright and revealing omissions: nothing is said about the involvement of Jozef Pilsudski in the attempt on the life of Alexander III, nor even is it admitted that another participant was his brother.

TADEUSZ SWIETOCHOWSKI
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VERA BROIDO. *Apostles into Terrorists: Women and the Revolutionary Movement in the Russia of Alexander II*. New York: Viking Press. 1977. Pp. vii, 238. \$15.00.

Women revolutionaries in the Russia of Alexander II merit a separate study. By the standards of contemporary Europe, the women were extraordinary. The majority from the privileged classes and unusually well educated (for women), they trickled into the movement in the 1860s and by the 1870s constituted roughly fifteen percent of the thousands arrested for illegal political activity. They participated in every phase of the movement, from peaceful propaganda and organizing through terrorism. Like their male comrades, who treated them, by and large, respectfully and as equals, they dressed as peasants and went "to the people," arranged escape attempts, carried bombs, wielded revolvers. Revolutionary women figured prominently in the mass political trials of the period, and impressed even unsympathetic observers with their courage and self-sacrifice.

Vera Broido, a very sympathetic observer, tells their story. Her book begins with a brief but competent sketch of the emergence of the revolutionary movement under Nicholas I, then traces in detail its evolution from propaganda to terrorism under Alexander II. She places the movement in its social and political context: amidst the inequities that first led people to rebel, and the reaction that drove them to ever more extreme measures. The book provides a useful introduction to Russian radicalism during this period.

As a work of scholarship, however, *Apostles into Terrorists* has several shortcomings. Broido has read and digested most of the published sources, but her account adds nothing new to our knowledge of Populism as a movement, nor to our understanding of the people who participated in it. More to the point, for a book that purports to be about the development of female radicalism, *Apostles into Terrorists* tells us remarkably little about women.

To be sure, the book does provide one chapter on "Feminism and Women's Education," and it properly includes women in its accounts of radical activities. But these are the only concessions to the particularity of women's experience. Broido makes no attempt to assess women's role in the movement. Her book does not discuss the way that female radicals coped with uniquely feminine problems—patriarchal attitudes among the people they tried to organize, for example, or the pregnancies and children that resulted from their love relationships. In short, the book treats women as if they were men. Even the vivid pen portraits that punctuate the book are as often of men as of women.

In her introduction to the book, Broido explains, "This is not, however, a feminist book. To assign to revolutionary women the narrow partisan role of feminists is to distort their position in the revolutionary movement and to diminish their contribution to Russian history" (p. vi). In discounting their feminism, however, Broido has neglected their womanhood as well. As a result, while this book provides a well-written and sympathetic introduction to radicalism in the reign of Alexander II, it is somewhat misleading to call it a history of women revolutionaries.

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DON W. CHENOWETH. *Soviet Civil Procedure: History and Analysis*. (Transactions of the American Philosophical Society, number 67, part 6.) Philadelphia: The Society. 1977. Pp. 55. Paper \$6.00

Civil procedure is, intellectually, not among the most stimulating branches of law. Dealing with technicalities of law enforcement, it is rather detached from the socioeconomic and purely human problems which pervade the substantive legal areas such as the law of contracts, torts, family, or criminal law. Its analysis is also less rewarding than that of criminal procedure—the latter is much more closely intertwined with protection of basic human rights and liberties. In spite of all this—which makes his merit particularly marked—Don W. Chenoweth's monograph, *Soviet Civil Procedure*, is an interesting and inspiring study. It contains a scrupulous and skillful presentation of a long and complex historic development: the primitive administration of justice preceding the reign of Alexander II; the liberal reforms of 1864 and the ensuing emergence of a modern legal, and especially procedural, system in Russia; the destruction of the system after 1917—first by the Krynlenkos of War Communism, and then, after a

brief breathing spell of the New Economic Policy, by Stalinism. Subsequently, Chenoweth traces, in some detail, the immense complexity of endless post-Stalinist reforms, each of them enacted easily and promptly by unchecked governmental legislative power. He presents many peculiarly Soviet procedural arrangements and institutions: for example, the enormous powers of the procurator who may, in defense of public interest, initiate, or join at any time, any civil case; the institution of people's assessors molded in a manner quite different from both the American jury and lay judges known to some European legal systems; the very active role of courts in collecting evidence; the review system composed of appeal, cassation, and ex officio supervision; and the Supreme Court guidelines—a unique kind of judicial lawmaking. Chenoweth also presents the basic principles and ideas governing Soviet procedure. Among them is the principle of "socialist legality" (which, however, has nothing to do with the rule of law as understood in America); the idea of the educative function of legal process (which reduces, to a high degree, to education for conformity); the principle of judicial independence (which is present on the law books rather than in the law in action); the principle of finding out the full truth as the basic notion underlying rules of evidence (which is, however, often compromised for political expediency).

On the whole Chenoweth's is a useful and competent study, of particular interest for comparative lawyers, legal philosophers, and historians. It illuminates a conspicuous historic regression—from the open and liberal legal institutions following 1864, to the legal "unsystem" of War Communism and the Stalinist years, to the more orderly but backward system of today.

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ALEXANDER BOYD. *The Soviet Air Force since 1918*. New York: Stein and Day. 1977. Pp. xix, 259. \$10.00

Histories of Soviet air power are surprisingly rare and, with few exceptions, of poor quality. This in itself makes Alexander Boyd's study, introduced by John Erickson, a welcome addition to the bookshelf. Using a wide range of recent Soviet memoirs and histories, as well as some older journals, he traces the growth of the USSR's air forces from 1918 to the present. Although the author also has brought a passionate interest to his subject, the result is unfortunately—and often needlessly—marred.

In part this results from Boyd's concentration on

the 1930s and early years of World War II. While this period is undoubtedly important for later Soviet policy, this is no excuse for devoting a mere seven pages to the years 1944-45. During that time the Soviet air forces came into their own and demonstrated the basic tactics that their frontal aviation may well apply again in any future European war. Boyd, who seems to share the Luftwaffe's low opinion of Russian aircrews' ability (if not of their courage), provides no real analysis of this vital period. Indeed, even his coverage of 1941-43 is far from satisfactory. Thus, for example, he apparently does not understand the structure of the Soviet high command and continually confuses the State Committee for Defense (GKO) with general headquarters (*Stavka*) and other bodies. As for the postwar period, thirty pages hardly do justice to thirty years of technical progress and administrative reorganization.

Boyd is best when discussing the events of the late 1930s and in describing the evolution of the Soviet aviation industry. Here he introduces the non-Russian reader to a mass of recent Soviet material. This makes *The Soviet Air Force since 1918* a useful supplement to R. A. Kilmarx's *History of Soviet Air Power* and Ray Wagner's recent translation of the official *Soviet Air Force in World War II*. Nonetheless, the reader is advised to use Boyd's volume with caution, for, despite his expertise, one must conclude that a successor to Kilmarx has yet to appear.

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NEAR EAST

JOHN P. SPAGNOLO. *France and Ottoman Lebanon, 1861-1914*. (Monograph, number 7.) London: Ithaca Press, for The Middle East Centre, St. Antony's College, Oxford. 1977. Pp. 339. £7.50.

John P. Spagnolo's book is neither a history of Ottoman Lebanon nor a summary of the French presence there. Rather, it is "an attempt to examine the relation of regional developments to external pressures in the crucible of politics and imperial diplomacy." His thesis is that French intervention in Lebanon, primarily in behalf of its Christian protégés, was motivated largely by a desire for advantage in the game of great power politics in Istanbul between 1861 and 1914, and further that French imperial policy retarded and distorted the political development of Lebanon.

The opening chapters describe the growth of French influence in Lebanon from the crusades through the events of 1860-64. European inter-

vention in 1860 to halt violence between Maronites and Druzes resulted in the establishment of the *Règlement*, the administrative document which guaranteed a degree of local autonomy for the Mountain. After modification in 1864, the *Règlement* came to be regarded by the numerically dominant Maronite Christians as a hedge against assimilation into the Ottoman Empire. Consequently it could be at best only a means to conservative political evolution or at worst an obstacle to change. This suited France well after 1870, when her international position was weakened by defeat in the Franco-Prussian War. The Quai d'Orsay felt its influence depended on preserving a privileged position for the Maronites, and this meant defending the *Règlement* against any attempt by the Porte or the *Mutasariff* (governor general) to tamper with it. France's defense of the *Règlement* was motivated by its own diplomatic interest rather than the long-term interests of its Maronite clientele. French policy thus "discouraged innovation in the internal political structure of the Mountain . . ." (p. 145).

After 1878, however, France inaugurated a new phase in its Eastern Mediterranean policies. Given the expanding influence of Britain, Italy, and Germany in the Ottoman Empire and the fear that Abd al-Hamid's authoritarianism was anti-French, Paris sought influence among religious and political groups in all of Syria. Good relations with the Maronite church and other Uniate communities became more important, but primarily as a stepping stone to the diffusion of French influence into geographical Syria. The growth of Arab nationalism and the resultant Arab-Turkish tensions seemed to offer an opportunity to Christians, especially the Maronites, to work for greater Lebanese autonomy. Maronite leaders, according to Spagnolo, hoped for an erosion of Ottoman sovereignty in the Mountain and looked to France for support. The French, however, refused to encourage Lebanese independence, which they viewed as contrary to their interests in Syria. Spagnolo concludes, therefore, that the famous *Règlement* revisions of 1912, clarifying and expanding certain aspects of local autonomy, were only modest steps in Lebanese political development. The amendments failed to satisfy those who viewed Ottoman political instability as an opportunity for radical change.

Spagnolo's thesis, clearly presented and forcefully argued, is based on extensive archival research in Paris, London, and Istanbul. If there is a weakness in Spagnolo's approach, it is his reluctance to distinguish clearly between France's Lebanese activities and its ambitions in greater Syria. Despite his conclusion that the 1912 reforms in the *Règlement* did not satisfy radical Lebanese

autonomists and irredentists, evidence suggests that France's Christian clientele was not wholly dissatisfied. The Maronite patriarch, for example, assured the Quai d'Orsay in 1914 that the Maronites would remain faithful to France. Furthermore, it seems beyond question that France's difficulties in dealing with Lebanese matters (modifications in the *Règlement* required agreement among all the European powers) encouraged Paris to avoid similarly internationalizing the Syrian question. This consideration explains France's drive to solidify her position in Syria through bilateral understandings in 1914. This matter aside, however, Spagnolo has rendered a service by focusing on a relatively neglected era in Lebanese history. Given quite recent developments in Lebanon, his book is timely indeed.

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DAVID KUSHNER. *The Rise of Turkish Nationalism, 1876-1908*. Totowa, N.J.: Frank Cass. 1977. Pp. x, 126. \$22.50

The intellectual origins of Turkish nationalism are to be found in the search by statesmen and intellectuals of the nineteenth-century Ottoman Empire for an ideological weapon of defense against the disruptive forces of European and minority nationalisms. Beginning with the vague concept of "Ottomanism," based upon equality of all Ottoman subjects and loyalty to the dynastic state, they subsequently experimented with a variety of religiously, linguistically, and racially based theories of nationalism.

David Kushner has turned his attention to this striving after national self-identity as reflected in the Turkish press during the reign of Sultan Abdülhamid II. He confronts the reader with the endless debates of Ottoman intellectuals over terminology, the role of Turks in history, the relationship of Ottoman Turks to their linguistic kin beyond the empire's borders, and the association of Turks with Anatolia, as well as with their attempts to identify a distinctively Turkish culture and to reform and enhance the Turkish language as the principal vehicle of that culture.

While the book's title accurately reflects the chronological perimeters of its contents, neither the title nor the author's preface indicates fully the other limitations in the scope of this study. Confining himself almost exclusively to an analysis of the press and publications of Istanbul and scarcely referring to those elsewhere in the empire and in Europe, the author has given us only one chapter, albeit a significant one, on the rise of Turkish nationalism, 1876-1908.

In recounting that chapter, Kushner fell victim to some of the same snares which entrapped the protagonists of his narrative—imprecise definitions and repetitive, circular, disjointed, or inconclusive arguments. Serious problems of organization are compounded by ones of presentation and style. Chaotic punctuation, particularly misplaced commas, obscures the author's meaning. Numerous inconsistencies in transliteration, spelling, and capitalization should have been eliminated by careful proofreading. Awkward, if not incorrect, translations and occasional factual errors can be noted. As examples of the latter, Ahmed Midhat was born in 1844, not 1855 (p. 17); the Academy of Learning was founded in mid-1851, not 1850 (p. 57); and Cevdet and Fuad were not assigned by the academy to compile a grammar but, rather, at that institution's inaugural ceremonies presented the already completed grammar to the sultan who ordered its publication as the academy's first work (p. 57). At least one major error appears in the index where both Ahmed Cevdet Paşa (1822-95) and İkdamcı Ahmed Cevdet (1862-1936) are included under the same entry (p. 121).

In spite of these shortcomings, Kushner's book is to be recommended for drawing attention to the ideological ferment going on in Istanbul even under the Hamidian tyranny which drove so many "Young Turk" intellectuals into exile in Europe.

RICHARD LEON CHAMBERS
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AFRICA

ALOHA SOUTH *et al.*, compilers. *Guide to Federal Archives Relating to Africa*. (Archival and Bibliographic Series.) Waltham, Mass.: Crossroads Press. 1977. Pp. xx, 556. \$75.00.

Because African history is a relatively new field of professional study in the United States, African historians here have had to work without some of the basic bibliographical and reference tools available to students of other fields. As an initial step toward remedying this, in the early 1960s the young African Studies Association resolved that a survey be made of Africa-related holdings in American archives and libraries. With support from the Ford Foundation and, later, the National Endowment for the Humanities, the National Archives and Records Service undertook such a survey on the association's behalf. This *Guide to Federal Archives Relating to Africa* is the first result, and a second volume on state, local, and privately owned archives and manuscripts is forthcoming. Together, and in combination with several other good, recently published guides and bibliogra-

phies, these two volumes will fill much of the void of solid reference works on materials in this country relating to Africa.

It took a dozen years to prepare this guide; the work that went into it is evident. It contains descriptions of the known written records, maps, sound recordings, and motion and still pictures having to do with Africa or its offshore islands located in the National Archives, regional branches of the Federal Archives and Records Center, and presidential libraries (though relevant materials in presidential and other personal papers in custody of the presidential libraries will appear in the second volume). These materials cover two centuries of American activities in Africa. Half the entries describe records of the Department of State—mostly correspondence and reports from foreign service posts; another tenth of the entries describe Department of Defense records; and the rest provide details of materials in the files of forty other agencies from the Department of Agriculture to the Works Progress Administration.

Entries in the *Guide* are arranged by federal agency and thereunder by record group, which usually correspond to a bureau of the agency. With each record group is a statement about its administrative history and function. Below this the arrangement is logical and the information for each numbered series of records, which includes an estimate of the portion of the total material that relates to Africa, is useful. Of necessity, descriptions of individual entries are uneven in depth. The compilers admit they could not describe voluminous dispatches of the State Department, for example, as fully as they could a Navy Department logbook. But on the whole the descriptions are as complete as one could wish. The book contains three appendixes to help make sense of State Department classification and filing systems, and it has valuable indexes to subjects, places, proper names, ships, and ethnic groups.

No review of this volume would be complete without mentioning that it is the first publication of the Crossroads Press, an enterprise the African Studies Association established over a year ago to publish scholarly materials relating to the Third World. African historians and many others will undoubtedly welcome Crossroads publications, all the more now that its first effort proves so useful. Undoubtedly, too, they will welcome the maturing of the new press, if age improves its copyediting procedures. Two glaring typographical errors in the first ten lines of this book's preface, along with a number of others scattered throughout, make such comment necessary.

Yet, neither typographical errors nor the book's healthy price should detract from any statement of

its potential worth. Quite simply, it is an invaluable research tool for students of Africa and, especially, African-American relations. Any library that wants to boast a complete African reference collection will have to have a copy.

DONALD R. WRIGHT

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LUCY MAIR. *African Kingdoms*. New York: Oxford University Press. 1977. Pp. 151. Cloth \$8.95, paper \$4.50.

To deal with African kingdoms, in the perspective of time and space, in such a short book is something of a tour de force; few could have accomplished this feat with greater success than Lucy Mair. Her book reflects the same characteristic breadth of scope, attention to detail, and clarity of exposition to which she has accustomed readers of her previous work, *African Societies* (1974). This is the best short introduction available to the dynamics of social change in African monarchical settings.

Mair's treatment transcends the frozen mold of the ethnographic present and introduces the reader to the various processes and mechanisms through which, historically, power has been accumulated by kings, and in time redistributed from the throne to the periphery. Of special interest in this connection is her discussion of "The Resources of Kingdoms," in which she shows the relationship between the handling of economic resources, including tribute, tolls, and taxes, and the structure of monarchical power. The fragility of power relationships between kings and chiefs, and chiefs and commoners, the factional struggles for the kingship, the structure of succession contests, and the transformations that have affected the office of kingship as well as the position of the officeholder under the impact of the colonial state are elegantly analyzed. The reader will be impressed by the sheer scale of anthropological and historical evidence the author brings to bear on the analysis of kingship, as well as by her ability to look at monarchical institutions from a wide variety of perspectives, and to move beyond the formal trappings of African kingships.

Some may regret, as I do, Mair's blissful unconcern for questions of methodology and conceptualization, her apparent inability to come to grips with the key issues raised in recent times by Marxist and functional interpretations of traditional forms of authority, and her all-too-superficial treatment of the processes of change initiated under the aegis of colonial rule. Although this is in a way a thin book, in both senses of the word, it

nonetheless has the merit of readability and comprehensiveness. There is no better introduction to an understanding of how African kings managed to build and expand their power base; it is to be hoped that Mair may eventually provide us with the logical sequel to her story—the decline and fall of African kingships.

RENÉ LEMARCHAND
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ROSS E. DUNN. *Resistance in the Desert: Moroccan Responses to French Imperialism, 1881–1912*. Madison: University of Wisconsin Press, 1977. Pp. 291. \$20.00.

Although based on mostly Western sources, *Resistance in the Desert* is presented not as a chapter of European military or diplomatic history, but as a chapter of African history. The foreign sources, however, are bolstered by oral data collected in Morocco. The book is a study of rural Moroccan reactions to the European conquest of their land between 1881 and 1912. It complements Kenneth L. Brown's *People of Salé* (1976), which clearly explains how Moroccans in an urban setting reacted and adapted to the French penetration and conquest of their land.

Ross E. Dunn has written a book about pastoral tribes and the inhabitants of oasis communities on the northern fringe of the Sahara. These people were subjects of the Sultan of Morocco, although they generally remained independent of the central government. Fragmented into ethnically differentiated Arabic- and Berber-speaking groups, they lacked strong central institutions or ethnic homogeneity. Thus they were typical of other fragmented African societies who resisted colonial conquest while lacking the institutions that might have helped them to be more effective in their resistance to foreign penetration. Therefore, as the author claims, this work is a model study of any number of African movements of resistance against foreign intrusions during the late nineteenth and early twentieth centuries.

Dunn's study of rural Moroccan resistance to French penetration and his explanation of why this movement failed is appropriately placed in the context of regional geography and of an extensive ethnographic description of the people involved. The author follows this up with an excellent account of the relationships of the Moroccans of the Saharan fringe to the Sultan's *maghzen*. These contacts, he argues, were much more extensive than earlier imagined or conceded. Finally, he deals with the impact of French penetration, detailing the rural Moroccan response to these military intrusions.

In this context, then, Dunn has little trouble

proving his two main theses, the first of which is that European military, economic, and political pressure led to a great deal of interaction between the inhabitants of the fringe area of Morocco and the central government of the Sultan, a government that could offer neither effective political leadership nor military aid. A second theme is that resistance to the French went hand in hand with trade, negotiations, and collaboration. The south-eastern Moroccans were neither resisters nor collaborators, but both. Thus they generally practiced the political art of the possible: resisting whenever possible, collaborating whenever defiance was pointless. These related theses, as convincingly demonstrated by Dunn, tell us much more about Morocco and Moroccans than earlier studies which used Moroccan history as incidental scenery for essentially European affairs outside Europe.

Resistance in the Desert should be highly recommended to African and Middle Eastern historians and anthropologists as well as to anyone interested in resistance movements. It is a sound and welcome addition to the scholarly literature on Morocco and on African resistance to European colonization.

ALF ANDREW HEGGOY
University of Georgia

TREVOR JONES. *Ghana's First Republic, 1960–1966: The Pursuit of the Political Kingdom*. (Studies in African History, number 14.) London: Methuen and Company; distributed by Barnes and Noble, N.Y. 1977. Pp. 366. \$18.50.

The central themes of this history of Ghana's first republic from 1960–66 are those of decay and inefficiency, qualities which alas seem to have infected the publisher and author of this book. Both the hardback copy sent me for review and the copy I purchased on my own account have broken at the seams and pages have slipped from the binding glue. I have rarely come across an academic work so incompetently proofread, with some sentences rendered obscure by misprints, for example, participate for precipitate (p. 118), and nor for not (p. 145).

I place emphasis on these defects largely because Trevor Jones seems to have set out in his book to highlight everything bad about Nkrumah's republic to the exclusion of any of its good aspects, which gain only brief mention in the final short chapter, "Nkrumah in Retrospect, 1966–74." Much of Jones' book reads more like a brief for the prosecution rather than the summing up of a judge. Thus we have a forty-two page chapter on corruption in Ghana under Nkrumah entitled

"The Fruits of Office," while less than half the space could have made the point that corruption was at a high level which, though the author does not remark upon it, has, if anything, been surpassed under the present regime. The space saved could have been devoted to Nkrumah's foreign policy.

Although Nkrumah did indeed leave Ghana in a sorry state when he was overthrown, he had made some very positive achievements in foreign policy: he made newly independent Africa's voice heard in a hitherto largely deaf comity of nations; he drew attention to the threat neocolonialism represented to the fragile independence gained by African countries in the sixties; he saw the need for a balkanized Africa to reorganize itself into larger groupings if it were ever to have real political and economic clout internationally—a Bismarck or Cavour before his time; he forced many Africans to consider socialist alternatives to the predominantly capitalist infrastructures that were the legacy of their colonial masters. Above all he and his country stood as symbols of the regained dignity of the black man, and as a result of Nkrumah's foreign policies not only many Ghanaians and Africans, but blacks of the diaspora, too, regained faith in themselves. All this has no part in Jones' story. Yet it is vital to an understanding of why Nkrumah, execrated in 1966, has subsequently been rehabilitated.

I am also doubtful of the wisdom of limiting this study to the period 1960–66. True, as the title indicates, it is a study of Ghana's first republic, but in fact it is much more a study of Nkrumah and the party he created. The changeover from dominion to republican status in 1960 was largely symbolic in terms of both Nkrumah and the C.P.P. The really significant date for both was 1957 when independence removed the final British constraints on them. Too much of what happened under the first republic has its explanation in these first three years of independence for the author to ignore them because they were not republican years.

Within these limitations Jones gives an incisive account of the organization of the C.P.P., its transformation into the single legitimate party, the factions within it, and the way Nkrumah played these off against each other. His account of the way development plans were "deformed" by political and venal considerations is good, as is his analysis of why both workers and farmers withdrew their support from the party. But even where he is at his best Jones cannot avoid the prosecution advocate's sneer, exaggeration or implication of felony when it is not there. Thus when reference is made to the fact that Nkrumah gave presents to a lady M.P., and to a priestess, the epithet "fair-skinned" is used in the case of the former and "fetish" in the

case of the latter, with obvious though not demonstrated implications. The Star Hotel where Krobo Edusei lodged his girlfriend for five years is described as "expensive," whereas those familiar with Accra know it is one of the more moderately priced hotels, and just the sort of place one would lodge a lady of easy virtue.

All in all this is a book which, though frequently stimulating and containing fruitful insights, should be read with some caution.

MICHAEL CROWDER
University of Lagos

TONY BARNETT. *The Gezira Scheme: An Illusion of Development*. Totowa, N.J.: Frank Cass and Company. 1977. Pp. 192. \$30.00.

The Gezira Scheme is a careful study of tenant farmers in rural Sudan. At the same time, it is a perceptive study of the incorporation of rural Africa into a world capitalist economy, and of the effects of that incorporation on the tenants themselves. On the whole, the study is competently done and clearly presented.

Tony Barnett focuses on the village of Nueila, situated in an irrigated agricultural project in east central Sudan. Concerned with the tenants in this project (the Gezira Scheme), whose origins go back to the early 1900s, Barnett explores their history and economy and their patterns of political, social, and religious organization. Relying on archival research, field surveys, interviews, and observations and compilation of labor and household budgets, he offers both a narrative description and an analysis. His major finding is that the tenants on this scheme, organized around cotton production, are usually in debt and that for many, the debt gets worse. In debt and excluded from effective political participation, the individual tenants become marginal to the larger economy (of the Sudan and of the world), more rural workers than peasants. Underdevelopment, not development, is occurring.

The major strength of Barnett's work is his success in linking observation with explanation. The commercial goals and assumptions that guide the Gezira Scheme management combine with organizational constraints and the requisites of irrigation technology to prevent tenants from becoming relatively self-directed peasants and active political participants. The Gezira tenants supply only labor, unlike peasants, who often supply land and sometimes capital and equipment. Since the labor required exceeds the family labor available (except for very short periods in the tenant's life cycle), each tenant must hire labor. But since hired labor is relatively scarce and relatively expensive, the

tenant must rely on credit to pay laborers. Hence, tenants face perpetual, and usually worsening, debt. Only those tenants with access to other capital, usually through kinship ties, can become creditors as well as debtors, or can escape debt entirely. Programs to promote wider popular participation in local governance have been stymied by Sudan's dependence on the revenue from cotton exports. That is, the national government, because of the importance of export revenue, is reluctant to jeopardize cotton production by encouraging broader local activism among tenants. As well, both local government and Gezira Scheme officials and the few rural capitalists in the Gezira area see broader participation as a direct threat to their own positions and as a result act to block local initiatives.

In short, the incorporation of the Gezira tenants into an expanding capitalist world economy, originally to satisfy the cotton needs of inefficient Lancashire manufacturers, led to a particular organization of production. That, in turn, led to a particular administrative structure, designed to produce cotton and preclude participation. That organization of production also led to particular class relations within Gezira. Both the organizational structure (which reflects national orientations and alignments) and the local class structure (which reflects the local contradictions) function to impoverish the tenants and to maintain structural arrangements to perpetuate that impoverishment.

Barnett is also successful in linking large and small scale theory. Imperialism, capitalist world economy, and organizational behavior are used to understand, and are themselves understood in terms of, the sociology of class and kinship relations, the political economy of the local setting, and the nature of individual decision making.

Despite his skill in making these links, Barnett does leave several major issues unexplored. For example, Barnett talks of class largely as an outcome, rather than a cause. He devotes little attention to the hired laborers. In fact, there is in this book barely a hint of organized activity at the local level, notwithstanding references to party officials, Communist cell leaders, and Muslim influentials. There is very little sense of collective movement here. A second problem is that Barnett's analysis of the national level—even as seen through the local prism—remains too thin to support the sorts of arguments he makes. Barnett attributes to the current government the intention to promote broader participation, but does not suggest why. He stresses the government's reliance on cotton revenue, but he does not balance that against the government's need for peasant support, whose lack might also jeopardize cotton production. There is

little clear sense of the current situation of the incorporation of the Gezira area into the Sudan as a whole.

Overall, Barnett's work is important. Despite its narrow case study focus, and perhaps in part because of it, Barnett makes a significant addition to work on underdevelopment in Africa and in the Third World more generally. His presentation, a careful blend of historical materials, local contacts and observations, and reports on individual cases, makes those major issues both very comprehensible and very personal.

JOEL SAMOFF
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SIMON AYACHE. *Raombana, l'historien (1809-1855): Introduction à l'édition critique de son oeuvre*. (Collection "Gasikarako.") Fianarantsoa, Madagascar: Librairie Ambozontany. 1976. Pp. 509.

Preliterate societies, or those on the threshold of literacy, are not supposed to nurture practicing literate historians and a biographical study of just such a historian would thus seem doomed never to get off the ground. Indeed, students of traditional societies in Africa and Madagascar have come to regard their written sources in general as works of "outsiders" while looking upon oral traditions and other nonwritten documents as "internal evidence."

Simon Ayache's *Raombana* is not likely to alter such views, but it should at least remind us that the past has a way of rebelling against impositions of the present. Raombana was a literate historian among the Merina, the largest population group in Madagascar. In 1820, when only a handful of the Merina could read and write, their king, Radama I (1810-28), sent nine youngsters to be educated in England under the aegis of the London Missionary Society. Raombana was one of them and he returned home after three years in London and another five in Manchester. By the time of his death in June 1855, Raombana had completed over eight thousand manuscript pages for an ongoing *History of Madagascar* and a *Journal* kept more or less regularly between November 1853 and April 1855. As Ayache shows with clarity, whatever else can be said about Raombana he emerges as *historien de métier* through both the rigorous search for sources and the application of critical method.

Raombana was certainly more than a westernized man of intellect who took a deliberate turn into the historical craft. Friend of Europeans, he was also an early nationalist facing the expansion of Europe into Madagascar. As servant of his government Raombana could be a detached observer, capable of independent thought at a time when his

society demanded the opposite. He was, in effect, a perfect insider. As scion of an old aristocratic family Raombana found no door closed to him in Imerina; as an official of the Merina Government he visited many sections of the huge island; and as secretary of the royal palace, he had unbroken access to official documents for a quarter of a century as well as the lasting trust of the "*véritable maîtres du pays*." In some hundred lucid pages Ayache places Raombana in context through biography (family, youth, education, adulthood in its public and private versions), history (changes within Imerina and Madagascar, Madagascar and Europe in the 1800s), and through intersections of both (role of witness, reactions to events and people, diplomacy and personal influence on royal decisions from an essentially powerless office). Another hundred pages discuss Raombana as the "first Malagasy historian," while seven documentary and most useful annexes double the volume's final length.

The forthcoming (critical) edition of Raombana's *History of Madagascar* is outlined in a forty-three-page table of contents (Annex V). Strictly speaking, it is not a history of Madagascar as a whole but of Raombana's own imperial state (Imerina), mainly in the first half of the nineteenth century, a time when Imerina gained supremacy in the Great Island. With this restriction, its six thousand pages of text are an insider's tour de force, a detailed excursion into contemporary history, and a marked departure from all traditional conceptions and forms concerned with the past as history. Raombana imposes a sense of chronological control, debunks a number of legends, develops a thematic approach to history, and is probably the first archivist in his society. With such a subject at hand it is to Ayache's credit that Raombana is not presented as a historian without flaws. He was an evangelist on behalf of "new ways," an aristocrat fearful of the "lower orders," and a moralist who often used the past for "higher ends." *Raombana* is altogether an important book that should be widely read.

RAYMOND K. KENT
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GERALD W. HARTWIG. *The Art of Survival in East Africa: The Kerebe and Long-Distance Trade, 1800-1895*. New York: Holmes and Meier Publishers. 1976. Pp. xiii, 253. \$24.00

The Kerebe are a relatively small people (presently numbering about 70,000) living off the Lake Victoria coast in Northwest Tanzania. In dealing with their development during the nineteenth cen-

tury, Gerald Hartwig has attempted to transcend the usual "trade and politics" paradigm of pre-colonial African history. Although his immediate subject is the impact of long-distance commerce upon a centralized African polity, he seeks to uncover basic patterns of social change, including not only structures of authority and economic organization but also patterns of epidemiology, demography, and belief in witchcraft.

Even for such a small area, this is an ambitious task and Hartwig must make use of a wide range of sources: written documents, oral traditions, archeology and linguistics. Ultimately it is the oral traditions (mainly from two informants) which constitute the central primary evidence, since the written evidence from Europeans treats only the last part of his period, and the archeology and linguistics are essentially second-hand. However, by imaginative and judicious comparisons of his materials with studies from other areas, where the topics he is interested in could be studied more closely, Hartwig is able to produce a very plausible and interesting account of the Kerebe experience.

The book's central argument is that the participation of the Kerebe in Indian Ocean trade during the nineteenth century presented at least as many threats to their security as it did opportunities for advancement. These threats were not directly external—the Kerebe location on an insular periphery of the major caravan routes protected them from conquest or slave raids—but rather the more pervasive phenomena of assimilating new social roles and new disease parasites. Hartwig concludes that the Kerebe were able to deal with these problems by developing institutions of clientship and witchcraft control which required some sacrifice of potential political and economic gain.

The issues which Hartwig addresses are important ones and he has made an important contribution to their elucidation. Nonetheless his work disappoints somewhat. A major problem is largely beyond the author's control: oral tradition, with its fixation on dynastic chronology, simply does not provide the kind of data needed to explore questions of social change beyond the rather suggestive manner to which Hartwig is restricted. Possibly some biographical data on nonroyal personalities, particularly those involved with the sorcery and innovative economic achievement to which Hartwig gives so much attention might have been included. But at best, as he notes, the information is always imprecise and thus cannot develop or test in detail, particularly in numerical terms, the propositions which are asserted.

In pursuing the general logic of his analysis, Hartwig often tends to be rather pedestrian and repetitious, citing arguments from various other works at great length but never quite weaving

them into a fully coherent formulation of his own. Ultimately the author seems somewhat ambivalent about why this whole discussion is important. He manages to relate the Kerebe case to parallel situations elsewhere in Africa and even in Tudor-Stuart England, all of which is useful, but he treats rather casually the central question of how we are to interpret African patterns of economic change. The statements he does make about the risks attached to such change and the consequent African efforts to develop institutional structures which will contain their impact point in a direction which I find acceptable. But for this kind of investigation to proceed further, it will require more convincing evidence and clearer exposition than is presented here.

RALPH A. AUSTEN
University of Chicago

T. R. H. DAVENPORT. *South Africa: A Modern History*. Buffalo, N.Y.: University of Toronto Press. 1977. Pp. xv, 432. \$6.95.

From within South Africa comes this new overview of the country's history. Its author, professor of history at Rhodes University, one of the country's four English-language "white" universities, explicitly places himself within the "liberal Africanist" school of South African historians and characterizes his work as "an interim report, an attempt to present the current state of knowledge about a society in rapid transition" (p. xiv). Yet his synthesis is likely to have far more permanence than his modest opening disclaimer would allow—in large part because of the nondoctrinaire fashion in which he has woven into his analysis the findings and viewpoints of all schools of South African history.

As the subtitle of the book suggests, its analysis concentrates primarily upon the twentieth century. A brief opening section of forty pages brings the account from the period before the arrival of the whites through an assessment of the Great Trek. The next hundred pages deal with the nineteenth-century struggles between and among whites and blacks for hegemony within the country, with particular chapters on the Mfecane, African politics, the Afrikaner republics, the British colonies, and competition for land. A short chapter of just over twenty pages outlining the making of the Union concludes the first part of the book, entitled "The Prelude to White Domination." In the second part of the book, "The Consolidation of a White State," approximately equal space of four chapters and seventy-five pages each is given to the 1910–48 period and to the post-1948 decades; in this part discussion shifts between white and black

politics with considerable detail offered upon the latter, as well as upon the efforts of the present Nationalist government to counter international challenges to its policies. Three brief summary chapters comprise the final forty-five pages of the text—discussing in turn segregation, the economic color bar, and recent historiographical controversies.

Except for the final three chapters, the book is generally organized in a straightforward chronological fashion. The focus of the analysis is political—how power was fashioned and broken in both black and white societies and how it was utilized through the unified South African state to secure Afrikaner hegemony and white domination. The particular strength of Davenport's presentation is the manner in which he keeps his line of argument clear through the detailed evidence he introduces to support it and the fashion in which he presents the conclusions of contrasting interpretations, ranging from Afrikaner nationalist to neo-Marxist, as part of his own integrated narrative.

When Davenport veers from his political focus in the three topical concluding chapters, grouped together in a section labelled "The Problem of Perspective," he further enhances the utility of his survey by sharply delineating the socioeconomic foundations upon which the present system rests and by succinctly summarizing the ongoing debate over the appropriate direction for South African history. The carefully balanced liberal position which this book represents will perhaps not provoke sharp controversy, but it does provide more than "an interim report."

SHERIDAN JOHNS
Duke University

JEFFREY BUTLER, ROBERT I. ROTBERG, and JOHN ADAMS. *The Black Homelands of South Africa: The Political and Economic Development of Bophuthatswana and KwaZulu*. (Perspectives on Southern Africa, number 21.) Berkeley and Los Angeles: University of California Press. 1977. Pp. x, 250. \$12.50.

Jeffrey Butler, Robert Rotberg, and John Adams claim to have provided a dispassionate examination of one of the central features of South Africa's apartheid system—the creation of ten self-governing and ultimately independent ethnically based states out of the scattered fragments of African-held land that constitutes some thirteen percent of South Africa's total land surface. Government policy has proclaimed these areas as the only places where Africans can have political rights. What the authors have produced instead is a potentially controversial book. The reason lies in their search for a resolution of the evolving crisis in South Africa

short of revolution. They endorse a policy of gradualism through which the bantustans (the authors prefer the euphemism "homelands") would serve as a way station "on the road to a restructuring of South Africa" (p. 231). Yet, the authors explicitly recognize that the bantustan policy is an integral part of the white regime's battle to maintain the developed core of the country as a white-run bastion.

Focusing primarily on two of the three key bantustans, Bophuthatswana and KwaZulu, *Black Homelands* is highly critical of the bantustans as they have developed up to the present. It depicts them as impoverished, overcrowded, and unhealthy entities with totally dependent economies. One of the clearest signs that the bantustans are client states with minimal autonomy is the near total dominance of white administrators and planners working primarily to implement programs initiated by the South African government for its own ends. Despite the dismal picture the authors present of the bantustans, they argue that some benefits exist in the scheme for Africans. The bantustans provide some Africans with modern political roles not hitherto available to them. Of particular importance are the legitimated platform and the new institutional base which have become available to a new group of African leaders. They now have the opportunity, absent since the suppression of the principal African political organizations in 1960, for developing a mass following in both the urban and rural areas, thus enabling them to wrest meaningful concessions from white officials and politicians. Hence, the book devotes extensive space to Lucas Mangope and Gatsha Buthelezi, the respective leaders of Bophuthatswana and KwaZulu.

Viewed in the context of the authors' predilection for a policy of gradualism, their approach of both sharply criticizing the bantustans and asserting the potentialities of these pseudostates for Africans emerges as a call for the South African government to enhance the credibility of apartheid. If South Africa's rulers genuinely wish to mute the black-white struggle for power and thus further entrench the white position, suggest the authors, they must take immediate cognizance of the bantustans' shortcomings and enable the Mangopes and Buthelezis "to deliver the 'goods'" in the form of substantive though marginal improvements to the population of the bantustans. "This is what political independence is all about" (p. 177). In short, *Black Homelands* is a blueprint for enabling the bantustans to provide stability, promote orderly change, frustrate revolution, and preserve in general the basic status quo in South Africa.

It is possible to dismiss this book on political grounds, for its arguments are essentially political

in nature. Historians, however, have yet another reason to be dubious about its conclusions. The weight of historical evidence suggests that elsewhere in Africa people were unwilling to continue accepting the disparities of colonial rule and acted to secure their independence. It seems unsound to postulate that Africans in South Africa will settle for less, especially in light of their long and protracted struggle for freedom. Moreover, it is highly questionable to envisage the entrenched white oligarchy, which for decades has insisted upon a total monopoly of political and economic power, devolving or even being capable of devolving any of that power to Africans. Simply put, *Black Homelands* will not stand up to rigorous historical criticism.

R. HUNT DAVIS, JR.
University of Florida

ASIA AND THE EAST

SARASIN VIRAPHOL. *Tribute and Profit: Sino-Siamese Trade, 1652-1853*. (Harvard East Asian Monographs, number 76.) Cambridge, Mass.: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1977. Pp. xviii, 419. \$15.00.

Sarasin Viraphol joins the ranks of John Fairbank, Chang Te-ch'ang, Chang T'ien-tse, John Wills, and others who have illuminated China's early modern relations with the outside world. Using European, Chinese, Japanese, and Siamese sources, he reconstructs the structure and extent of the Siam-China trade from 1650 to 1850. The work is particularly interesting because it focuses not on East-West exchanges, but on intra-Asian junk trade which flourished at a time when European merchants had difficulty penetrating the East.

The very existence of this junk trade reflects forces for change indigenous to Asia. Rulers and elites often could not accommodate these forces to their preferred world. But they had no choice. The wealthy craved the thousands of deerskins, the tons of pepper, and the holds full of ceramics, textiles, fans, and woods. The needs of state required copper, iron, and saltpeter. A growing eighteenth-century Chinese population demanded rice imports and places to emigrate.

But how much trade could Japan's shoguns tolerate before the daimyos' wealth, audacity, and subversive Christianity toppled them? How much commercialism, smuggling, and squeeze could the Ch'ing emperor put up with before the Confucian moral and social order collapsed? At what point would commerce undermine the Siamese king's

realm, where noble simply extracted labor and goods from peasant? These ruling institutions responded according to the specific circumstances. It seems possible to see elements of both stagnation and dynamism in the resulting compromises.

Although the book is full of material relating to the change in early modern Asia, the author himself does not present a sustained discussion of it. His position is ambiguous. On the one hand, he seems to say that Asian institutions were a bad thing in that they frustrated Weberian, capitalistic urges (pp. 5-8, 123-24). On the other hand, in Siam, Chinese tax farmers, secret societies, commercial agriculture, and resistance to Western competitors are portrayed as a good thing (pp. 211, 215, 220, 244-46).

The strength of the book lies in the delineation of the junk trade. In the seventeenth century it developed under cover of Siamese tribute missions to the Manchu court. Simultaneously private trade developed, although it was usually illicit. Most of the participants were Chinese: local merchants, sailors, merchant-officials attached to the Siamese court, and Ch'ing officials. In the early years the emperor indulged the trade as the price for maintaining tributary relations. He later countenanced it to encourage rice imports. Finally it withered in the nineteenth century because of Western ships, new entrepôts like Hong Kong, and the decline of the Ch'ing. The author reconstructs how the tribute trade worked in its heyday. Not only were the Siamese tribute ships permitted to sell their "ballast" and purchase a cargo for sale at home, but also they were allowed to go back while the mission was in Peking and return for the mission later—with a new "ballast." Where possible the author attempts to quantify the number of junks, the volume of trade, and the rates of return. He touches on many matters of interest: sixteenth-century Japanese participation in the Siam junk trade, the role of Chinese in Siamese society, the shifting fortunes of Chinese dialect groups in Siam, etc.

The book includes an excellent bibliographical essay and careful notes, index, glossary, etc. It supplies some important additional pieces to the puzzle of early modern Asia and therefore deserves to be received as a welcome contribution to the field.

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JAMES HAYES. *The Hong Kong Region, 1850-1911: Institutions and Leadership in Town and Countryside*. Hamden, Conn.: Archon Books, 1977. Pp. 289. \$17.50.

No other researcher knows rural Hong Kong as James Hayes does. And no area, Taiwan excepted, offers a researcher the diversity of resources for Chinese local history that rural Hong Kong does. Happily, we now have in monographic form the results of Hayes' many years of study of land records and other raw data, translations of published gazetteers, and interviews with old residents. The completed study, though narrow in geographic focus, is broadly significant in terms of problems in modern Chinese history.

Hayes is concerned with local leadership and political organizations. His method is to focus upon six communities: three market towns and three rural villages or village complexes. According to the conventional wisdom of Fairbank, Hsiao, and Balazs, the key to local leadership and organization in late imperial China was the gentry. Holding degrees through the national educational qualifying examinations, gentry frequently resided in local towns and in the countryside where they functioned as unofficial political leaders below the lowest level of formal government. Contrary to this view, Hayes finds little if any gentry presence in his localities; instead, the leadership consists of clan elders, merchants, and other prosperous but nongentry individuals. Their leadership is expressed through clans, "same-district" or "original hometown" associations, charitable societies, and, in the market towns, merchant-led neighborhood associations, the *kai fong*. Hayes argues convincingly that these nonintellectual elites were perfectly capable of governing their own affairs and needed no gentry or formal governmental interference.

Although his argument echoes the findings of Kuhn and Schoppa, it is nicely laid out in ways that suggest fruitful lines for further research. Two are obvious. One is the subject of local control by the central government; if gentry were absent, how *did* the center exert its control? A second is: can we define for China as a whole a stratum of local elites—semi-intellectuals, perhaps—below the level of the nationwide gentry elite, and, if so, what role does it play in social mobility? Hayes also refers his findings, in passing, to research on the overseas Chinese. Typically, nineteenth-century overseas Chinese communities were gentryless, yet functioned perfectly well, generating effective merchant leadership and a wide variety of associations. Hayes' findings suggest that we need to modify common conceptions about how overseas Chinese formed associations. It appears that the formula was not always one of drawing upon very general organizational models from China and adapting them to local situational demands. Many overseas Chinese associations, it appears, have been almost exact duplications of institutions found in China.

Despite these good points, one feels a sense of incompleteness and disappointment about this book. Hayes is right to raise the question of whether the semiautonomous "village republics" mentioned in early twentieth-century writings about China actually existed. But to address that question his material must be combined with the substantial literature from the Japanese tradition of *kyodotai* study, which attempts to find corporateness in irrigation and other associations in traditional Chinese villages. Regrettably, that effort is not made here. It is even more surprising that there is no discussion of secret societies in a book that deals with local leadership. It is understandable that Hayes' informants would wish to soft-pedal discussion of the topic, but rural Hong Kong is adjacent to a part of China where secret societies were extremely active and powerful in the late nineteenth century. No discussion of South China's local leadership and its political implications can ignore them.

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GAVAN MCCORMACK. *Chang Tso-lin in Northeast China, 1911-1928: China, Japan, and the Manchurian Idea*. Stanford, Calif.: Stanford University Press. 1977. Pp. vi, 334. \$16.50.

For more than a decade before his death in 1928, Chang Tso-lin dominated the vast and wealthy region of Manchuria, and participated decisively in the political and military struggles in north China. He was the most powerful single warlord in China, but very little has been published about him in English. Gavan McCormack's detailed and well-documented study of Chang's career thus fills a long-felt need, and provides a welcome Manchurian dimension to our view of militarism and politics in the early republic. Even more important, this book offers much information about hitherto little-known aspects of Japanese imperialism in China, particularly in Manchuria.

Many studies of Japan's China policy have pointed out that "Shidehara diplomacy," the policy promoted by Shidehara Kijuro during the mid-1920s, rejected the notion of Japanese intervention in China. McCormack, on the contrary, argues that the basic principles of Japan's China policy remained essentially unchanged from 1918 through the 1920s. Japanese leaders distinguished Manchuria from the rest of China; they thought of Manchuria as separate from—and thus separable from—China. While they publicly proclaimed nonintervention as a national principle, the substance of Japanese policy remained firm and constant, and it was to maintain Japanese interests in Manchuria at all costs.

McCormack bases his study on English, Chinese, and especially Japanese materials, and supports his argument with very convincing documents from Japanese sources. These include not only explicit policy statements, but much evidence about specific instances of Japanese intervention in Chinese political-military struggles during the years of Shidehara diplomacy. For example, McCormack describes Japanese active support of Chang Tso-lin during the Second Chih-Feng War, in 1924, and uncovers a number of details about Japan's role in bribing Feng Yu-hsiang to defect from the Chihli forces. He also analyzes exhaustively Kuo Sung-ling's rebellion against the Feng-tien clique in 1925, providing the most comprehensive discussion in English of that ill-fated venture.

The cases of Feng and Kuo illustrate the critical support that the Japanese on occasion provided to Chang. Yet that support was given in spite of Chang's Peking ambitions, not to encourage them. In Japanese eyes, both Feng and Kuo had Bolshevik connections, and that created a strong motive for Japanese aid to Chang. In any event, the Japanese feared their interests would suffer if Chang were defeated. But the Japanese did not want Chang to take part in the warlord struggles in north China. They hoped he would devote his energy and money to maintaining peace and stability in Manchuria, so that Japanese lives and investments would be safe. The Japanese repeatedly urged Chang to stay at home and mind his Manchurian business. But Chang was not a passive puppet of Japan, and he exercised as much independence as he could under the circumstances.

This book also provides interesting material about Chang's administrative and economic policies inside Manchuria. It thus adds to the lamentably sparse literature about warlord governments. In treating this subject, McCormack highlights the differences between Chang's oldest supporters from his bandit days and the newly educated officers and officials who provided much of the expertise in the Manchurian administration during the 1920s.

As McCormack acknowledges, Chang Tso-lin remains in the end a shadowy figure. The sources simply do not reveal much about his personal life, although the author sketches a very lucid account of Chang's public career. In the final analysis, however, it is the Japanese angle that is the main focus of the book and its chief contribution. It gives us the most detailed picture yet of foreign involvement with a Chinese warlord.

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PETER SCHRAN. *Guerrilla Economy: The Development of the Shensi-Kansu-Ninghsia Border Region, 1937-1945*.

Albany: State University of New York Press. 1976. Pp. xvi, 323. \$20.00.

Peter Schran has heroically attempted in this volume to assemble as much quantitative data as he could find about the economy of the Shensi-Kansu-Ninghsia Border Region in northwest China (hereafter SKN) during the years of the Sino-Japanese war, when it was the principal base of the Chinese Communist movement. On the basis of this inevitably incomplete and often inconsistent information—the exigencies of wartime, the “primitiveness” of SKN’s economy and its statistical system, the secretiveness of the Communists about some critical matters, and the accidents of later history preclude any fuller documentation—he offers as thorough an account as we shall probably ever have of the development of SKN’s “guerrilla economy.”

Schran first describes the “benchmark” condition of SKN (a “backward” economic region with a population of ca. 1.5 million engaged primarily in growing millet and wheat) before the influx of Communist forces at the end of the Long March from the Soviet areas of southeast China. He then traces the evolution of economic policy, which he sees as growing directly out of the failures and successes of the previous Kiangsi Soviet experience. The organizational structure of SKN he describes as a dualistic pattern which left much of the previous structure, especially in agriculture, intact but superimposed upon it “institutional” households (notably the Eighth Route Army contingent which engaged in production as well as warfare), a small state-operated sector for strategic materials, and a more substantial cooperative sector devoted principally to peasant marketing, commerce, and handicrafts. Government labor-mobilization policies were able to increase employment in import-substituting industry and home industry (principally cotton textiles), in export-supplementing salt production, and in the public sector (this last to an unintended and sometimes unacceptable degree). Over the period 1937–45, the output of food crops in SKN probably increased moderately, primarily as a result of large-scale land reclamation; salt output grew significantly but was constrained by the Nationalist authorities’ blockage of the Communist-controlled areas from 1941, which limited the potential market; and a substantial but incomplete degree of autarky (smuggling remained an important source of supply) was achieved in basic consumer goods, while metal processing and armaments manufacture lagged behind. The Nationalists’ blockade of SKN, after a period of free trade, plus Nationalist aid during 1937–40, presented the Communists with a serious balance-of-trade problem which was met only in part by efforts at autarky. Deterioration in the

terms of external trade precluded a balanced budget. The border region bank issued more notes to finance the deficits incurred, leading to a very sharp inflation whose untoward consequences, however, were limited by the small extent of private money economy in SKN (farm households continued to produce mostly for their own consumption rather than for the market).

Overall, while the SKN economy remained “backward” and poor, Communist economic policy during the 1937–45 was able to sustain average levels of income and living in the farm sector while reducing income differentials substantially; increase nonfarm production and income; support the Eighth Route Army troops relatively well; sustain limited military operations against the Japanese; and train and export military and civilian cadres to other areas of north China behind the Japanese lines, where more than a dozen other base areas—less secure while the war lasted than SKN, but ultimately the bulk of the territory and population controlled by the Communists in 1945—were being developed. (This last, I note, Schran describes as “the SKN border region’s most important accomplishment.”)

I have no problem with any of these conclusions; nor do I suspect that they will trouble very many other readers. In their moderately optimistic appraisal of what the Chinese Communists were able to accomplish in “Yenan” they confirm the observations of the limited number of first-hand journalists’ and diplomats’ accounts of these years in SKN, which have long been available, while eschewing the myth-making of the politicized historiography of the People’s Republic of China. While the Yen’an years are indeed something for a Chinese to look back upon with pride, nowhere—bless him—does Schran hint that the experience of running a “guerrilla economy” in SKN, with its population of 1.5 million peasants, has any relevance to the massive economic problems faced in 1978 by Hua Kuo-feng and Teng Hsiao-p’ing who lead nearly one billion Chinese in the post-Mao era.

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WANG GUNGWU. *China and the World since 1949: The Impact of Independence, Modernity and Revolution.* (The Making of the Twentieth Century.) New York: St. Martin’s Press. 1977. Pp. viii, 190. Cloth \$16.95, paper \$4.95.

This slim volume is one of the series, “The Making of the Twentieth Century,” which focuses upon

various significant and sometimes controversial historical events or themes of the present century. A historical study of the first quarter century of the People's Republic of China is an eminently suitable topic for the series and serves an obvious need: Western historians of China seem to have developed an unspoken consensus that Chinese "history" ends in 1949, and so have left study of the People's Republic to political scientists, journalists, and the CIA. Wang Gungwu's book focuses on "change," and he has clearly set out to apply the historian's craft to the post-1949 period.

The book's structural integrity is built upon a three-legged interpretive framework that groups change around the themes (or goals) of independence, modernity, and revolution. In developing these themes, the author consciously reacts against both the Stalinist sovietization model of Chinese change fashionable in the 1950s and the more recent tendency to emphasize the "Chineseness" of everything China has tried to do. Yet he does repeatedly and consistently stress the Chinese distinctiveness of these themes and the Chinese view in general. Perhaps less consciously, he uses these themes to discredit the vague image in the West of an aggressive, expansionist China in the grip of "antimodern" Maoism. For example, Chinese "independence," for historical and cultural reasons, does not mean simply independence as defined in international law; it means freedom that can only be guaranteed by China's equality in strength with the strongest (i.e., the super powers). Yet, in accord with the Chinese view, this is not equivalent to *being* a super power. Thus, Wang explains Chinese actions, which some have interpreted as aggressive or expansionist, as being manifestations of the Chinese impulse to "independence." Similarly, his treatment of modernity stresses the rejection of both Western and Soviet criteria and the ongoing development of a uniquely Chinese modernity. He admits only that Mao's way "came perilously close to restoring the dichotomy between a Chinese social morality and certain alien scientific deals" (pp. 142-43). Although "revolution" is obviously closely related or even part of the first two themes, the word seems to refer to a process of continuing an unending struggle toward an undefined "good" society in the unforeseeable future. While still a direct descendant and continuation of the great Western revolutions (including the Soviet), it too is distinctively Chinese.

Wang has made a contribution to the genuinely historical study of post-1949 China. His treatment of foreign relations (i.e., independence) is especially perceptive and full. His overall narrative, however, is a bit too incomplete and sketchy to serve satisfactorily as a basic text for the newcomer. The book's difficulty is that it is not quite

an interpretive essay nor quite a survey textbook. It is far too brief to serve as a reference compendium of facts and figures. Nor does it attack in depth the fundamental theoretical problems or create a historical typology that might guide future thinking and research on the period. Despite these limitations, the nonspecialist can read the book with profit, and the themes Wang handles in the limited space at his disposal deserve further exploration.

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BILL BRUGGER. *Contemporary China*. New York: Barnes and Noble. 1977. Pp. 451. \$22.50.

The recent spate of proclamations by liberal journalists and academics regarding the death of the Chinese Revolution is reminiscent of earlier, equally convincing obituaries of the Chinese Revolution. Although it is hardly clear that this greatest revolution in world history remains alive and well, it is too early to be certain whether there is a 'historical corpse upon which we can perform the professionalized fragments of a post mortem. Discretion, therefore, dictates that we re-view the history of China's body politic since it was given shape in the 1940s. For such purposes we are fortunate to have at hand Bill Brugger's latest book, *Contemporary China*, which is far and away the best textbook I have seen to date on the Chinese Revolution.

Reading it reminds us anew that this latest round of obituaries may signal not the demise of revolution but only the reassertion of a consolidation phase in a revolution that has developed through a series of dynamic cycles. And, in any event, the book rekindles the realization that revolutionary forces in China, with a history of fierce struggle, may not be so easily domesticated.

The organizing thesis of Brugger's profoundly informed historical interpretation of the thirty-one years between 1942 and 1973 is that this astonishing period of revolution is best understood in terms of nine progressive cycles of radicalism, accelerated radicalism, and consolidation. The cycles, happily, are presented concretely and dialectically, rather than abstractly and mechanically. Thus, in his preface, Brugger, a senior lecturer in politics at the Flinders University of South Australia, writes: "the relationship between central leadership and mass initiative is not causal but *dialectical* . . . Both . . . ideally interact to produce policies that are not simply the effect of pressure from different directions nor a compromise solution but something which is qualitatively different" (p. 16).

Consequently, although Brugger has a lot to say about the power struggles among leaders in each cycle, the Chinese Revolution is understood neither as an elite power struggle, nor as manipulation by an elite of the political process. The struggles that fuel the cycles have been, in essence, about how to overcome "the division of labor inherited from the old society and inherent in any transitional period" or process, like socialism (p. 16). They have been fought over the correct line for advancing socialism and achieving Communism. The main protagonists within the leadership have been "conservatives" like Liu Shao-ch'i, economic determinists committed to a Soviet model of development, who have sought "to process people into socialist society" (p. 14); "ultraleftists" like Ch'en Po-ta, "idealists" committed to a "faith in . . . the power of human will. . . divorced from reality" (p. 362), who in their impatience and intoxication with "instant communism" "try to hector people into becoming socialists" (p. 14); and "radicals" like Mao himself, dialectical materialists committed to the Yen'an model, who "see socialist transition as a process of alternating order and disorder in which economic development and social change interact dialectically" (p. 14).

In his account of these cycles Brugger divides the book into nine, chronologically arranged, chock-full chapters. The cycles and chapters generally correspond to conventionally accepted periodizations, such as the Great Leap Forward (1957-59) and the Socialist Education Movement (1962-65). What is unusual is the extent to which Brugger successfully communicates how the Chinese, in their own terms, understand their own struggles. Since he takes "very seriously what the Chinese say they are doing" (p. 14), he wastes less time than most translating *their* understanding into our world view.

Nowhere is Brugger's approach more fruitful than in his analysis of the ultra-Left as a meaningful political category. In contrast to most analysts of contemporary Chinese politics who do not take the category seriously and, instead, tend to treat its members primarily as scapegoats after the fact for failures of leaders' radical initiatives, Brugger understands that the category, although not "static," also is not "arbitrary." The ultra-Left is defined in each period and situation in terms of "people's consciousness" (p. 310), "mass reaction" (p. 317), and "the Mass Line" (p. 357). By taking the ultra-Left seriously and distinguishing it from the radicals, Brugger shows that the dissolution of the ultra-Left following the Cultural Revolution does not in itself effectively constitute a repudiation of the Cultural Revolution launched by the radicals. Such distinctions generally escape our Pekingologists.

Thus, in recent reports the campaign against the Gang of Four has been taken too easily as a wholesale repudiation not only of "the gang" and its allies, but of the Cultural Revolution and, more broadly still, of the Yen'an model embodying Mao's revolutionary tactics and strategies. This conclusion is premature at best. It also is predicated on a characteristically personalized, static, and undialectical vision of Chinese politics, in which China's leadership is dichotomized into unchanging "pragmatists" like the late Chou En-lai, Teng Hsiao-ping, and Hua Kuo-feng on the one hand, and eternal revolutionary zealots like Mao Tse-tung and the members of the Gang of Four on the other.

Whether the obituaries of the Chinese Revolution are proven correct by developments in China, Bill Brugger's *Contemporary China* is a lucid reminder that the struggles of the Chinese Revolution cannot be appreciated by such reductionist visions. If Brugger himself has some difficulty making sense of some of these struggles in the early 1970s, how much more difficult must it be for journalists and less understanding and less informed scholars to deliver instant analyses of the events of the past eighteen months?

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DAVID M. RADDOCK. *Political Behavior of Adolescents in China: The Cultural Revolution in Kiangchow*. (The Association for Asian Studies, Monograph number 32.) Tucson: University of Arizona Press, for the Association for Asian Studies. 1977. Pp. vii, 242. Cloth \$8.95, paper \$4.50.

Chinese adolescents in 1966-68, their schools closed, had to decide whether and to what extent they would participate in Cultural Revolution politics. They could become militant activists, leading Red Guard groups in writing wall posters, criticizing local authorities, seizing power, and battling rival factions; they could follow along passively; or they could abstain entirely. Investigating this political choice, David M. Raddock interviewed thirty-five recent émigrés from China in Hong Kong in 1971-72, spending eight to sixteen hours with each. All were male, Cantonese, aged 17 to 25; most were from urban, educated families, and all but two or three were of nonproletarian background, that is, their fathers had been "class enemies" (capitalists or Nationalist army officers, for instance) or petit bourgeois (typically teachers or professionals). Raddock concludes that their degree of Cultural Revolution participation was an individual choice which can be explained largely by the interaction of two factors.

Most important was their psychological relationship with their fathers: "student attacks on authorities often represented an extension of earlier family conflicts and an attempt to resolve them" (p. 10). To oversimplify, he posits that those who had submissive "vertical" relationships with strict authoritarian fathers were apt to lack the self-confidence and independence to rebel; they suppressed their anger against authority, identifying with their fathers often by devoting themselves to academic study rather than politics. Those from more open, equal, "horizontal" families, were more likely, if other conditions were favorable, to play out their competitiveness with their fathers by becoming Red Guard activists or participants.

The other key factor was their class label, or more precisely, the way they were treated at school because of it. Some students of class-enemy status had early in primary school felt a hostility from teachers and classmates which led them to identify with their politically backward fathers; they were apt to abstain from the Cultural Revolution on the grounds that political participation was useless for people of their background. But most of those of nonproletarian background, interestingly enough, had not been discriminated against in school until the mid-1960s. Then suddenly ostracized and denied admission to the Young Communist League or upper middle school or university, they were filled with resentment against what they perceived to be unjust treatment, and in the Cultural Revolution were apt to become activists, seizing the opportunity to get back at teachers and student leaders of proletarian background. Indeed Raddock thinks this social class issue was at the heart of Red Guard factionalism, the more radical groups being composed mostly of students of nonproletarian background (who were a majority in middle schools and universities) struggling against the privileged proletarian children, who were more restrained in their attacks on authority.

The great importance Raddock attaches to the father-son relationship—much of the book is concerned with analyzing the psychological dynamics of his subjects' relations with their fathers—is not always fully convincing. Why, for example, must Red Guards, seeking revenge against teachers who had picked on them, be explained as adolescents rebelling against "father-surrogates"; and are mothers, who are hardly mentioned, really of no significance? Raddock says the importance of fathers was an unexpected finding, yet one wonders if he was not predisposed to it, in the questions he asked and topics he pursued in interviews and by political culture theory that stresses the influence of childrearing on later political behavior. There is also the unavoidable problem, of which he is fully aware, regarding the extent to which one can gen-

eralize about China as a whole on the basis of a refugee, and in this case male, urban, and largely upper-class sample. Stylistically, this work, which was a Columbia political science dissertation, suffers from social science ponderousness and inelegance; many a sentence about "continuum of change" or "the seepage of environmental change into the family socialization process" must be read twice to be fathomed. The value of the book is in the fascinating glimpses Raddock gives of his subjects coming of age in recent China: of politically enthusiastic young people becoming alienated by hostile treatment based on their fathers' occupations before 1949; of children caught in a conflict between family expectations and social values taught in school; and of the forces tending to make the strict old-fashioned Chinese family more open and equal. Indeed his suggestion is intriguing that "horizontal" father-son relations may be more common on the mainland (about half his sample, admittedly atypically modernized) than in Taiwan: is Communist China producing self-reliant and individualistic personalities more than capitalist Taiwan?

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JAGADISH NARAYAN SARKAR. *A Study of Eighteenth Century India*. Volume 1, *Political History (1707-1761)*. Calcutta: Saraswat Library. 1976. Pp. xvi, 477. Rs. 60.

Jagadish Narayan Sarkar is well known for his extensive researches on medieval India, particularly on the Mughals. Characterizing the eighteenth century (1707-61), the theme of this college-level textbook, as the age of decadence, "a pervasive, tragic decadence," the author then narrates the political history of the successor states of the Mughals which arose in various regions of India: the Marathas, the Jats, the Sikhs, the Rajputs, the state of Hyderabad ruled over by the Nizam, to name a few. The author's sympathy, as with so many modern historians of medieval India, lies with the fallen Mughal empire, and this biased view of the eighteenth century has colored many of his observations. The rise of British power in India has been ignored, since their presence was not fully felt until the late 1760s.

The Marathas were at the forefront of the eighteenth-century struggle for supremacy in India. The author therefore has, quite rightly, devoted one third of the book to describing them. Also, while mentioning the histories of the other regional states, he clearly indicates their intricate network of relationships with the Marathas. Sarkar, however, subsumes the Marathas under part two of his book: Hindu revival. One doubts whether in the

context of the eighteenth century, which was the century of Hindu-Muslim accommodation, the dichotomy of Hindu-Muslim confrontation is an appropriate framework for the study of the political history of that period. Eighteenth-century wars in India were not fought on the bases of religious differences. In dealing with the Marathas as with the Rajputs, Sikhs, and the Mughals, Sarkar relies mostly on such standard secondary works on the subject as those written by Sardesai, Sir Jadunath Sarkar, and others, even though he has given twenty pages of exhaustive bibliography which includes many original sources. The bibliography, incidentally, is full of inadequacies. In a textbook of this nature, the author should have given full bibliographic entries which should have included: author, title, publisher, and the date and place of publication; there is a complete disregard for such a standardized format.

The book is a carefully described "catalogue" of political events connected with the regional Indian powers. There are occasional analytical comments, but generally speaking the volume is full of worn-out clichés and the views of other modern historians writing on that period. Still, the book is useful because it is one of the very few studies which deals, both cumulatively and individually, with various politically viable successor states of the Mughals from 1707 to 1761.

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JOHN PEMBLE. *The Raj, the Indian Mutiny and the Kingdom of Oudh, 1801-1859*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1977. Pp. 303. \$15.00.

John Pemble's book (whose title may seem to put the cart before the horse) is a broadly based study of Oudh, one of the rickety successor states of the Mughal empire, from the treaty of 1801, which placed it under British protection, down to the end of the Mutiny, which its annexation in 1856 did a great deal to provoke. He has done his work well, drawing on much archival material as well as the existing literature, to which Indian scholars lately have made notable additions. There are helpful maps, and illustrations further enliven a well-written book, clearly by a man of many-sided interests, including Urdu literature and the arts.

There is an appreciative though critical account early on of the culture of Lucknow, that luxurious capital of a decadent kingdom. Turning to the primitive countryside surrounding the glittering city, Pemble makes good use of R. G. Fox's study of Rajput clan-feudalism in showing how, under weak central control, the big landholders or *taluqdars* with their forts and retainers were getting the

upper hand. About the condition of the cultivators, under the double burden of feudal lord and profligate court, contemporary evidence is discordant. Pemble heavily discounts Sleeman's lurid report and points out that the peasantry, whatever its grievances, showed no inclination to abscond into British territory. As to the relationship between a greedy, overbearing British power and a feebly vicious native government, the record is unedifying. An interesting fact that Pemble brings out is the repeated insistence of governor-generals on large loans from Oudh to help to finance their wars. Much light is thrown on the working of the Residency system, and the shift about 1830 from easy, sometimes disreputable familiarity between Resident and ruler to a stiff, unbending aloofness.

Pemble is a military historian first of all, and this adds to the value of his narrative of the fighting of 1857-58 in and around Lucknow. One would have welcomed an estimate from him of Engels' commentaries on it at the time. British commanders show up badly, as bunglers and, too often, sadists. On the rebel side the links between the sepoys, a great many of them recruited from Oudh, and the petty feudal strata are emphasized; it is "inappropriate to speak of a 'peasant' revolt" (p. 188). There is a careful inquiry into the part played by the *taluqdars*, leading to the conclusion that most of those who, for a time, were active in the rebel camp were pushed into it by their followers. In the end they and the British came to terms at the expense of the peasantry. Whether or not Pemble is right in considering that the government had no alternative, India was to pay a heavy price for it down to the last days of the Raj.

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VED MEHTA. *Mahatma Gandhi and His Apostles*. New York: Viking Press. 1977. Pp. xi, 260. \$14.95.

V. S. NAIPAUL. *India: A Wounded Civilization*. New York: Alfred A. Knopf. 1977. Pp. xi, 191. \$7.95.

When anyone, student or specialist, studies the complex history and civilization of the South Asian subcontinent, it is necessary to confront a network of perceptions, stereotypes, and expectations. Almost invariably, one must deal with a civilization as intricate as that of South Asia by filtering diverse experiences through these stereotypes. If one approaches Indian history from a negative perspective, the result will be a self-fulfilling expectation; the reverse of this is also common and is the origin of the naive orientalist's fascination with "the East."

In the books under review, two distinguished authors have observed Indian culture and have

drawn lessons based on their own preconceptions. For V. S. Naipaul, the negative aspects of South Asia have led him to brand India as "a wounded civilization." Ved Mehta, on the other hand, has adopted a more sympathetic view of India and of modern India's "founder," Mohandas Karamchand Gandhi.

Naipaul's *India: A Wounded Civilization* is a compilation of essays which appeared originally in the *New York Review of Books*. As an "overseas Indian," Naipaul brings many of the emotional reactions of the alien observer to his experience of India. He finds the contradictions of contemporary India—and especially of the urban elite—irreconcilable because his classificatory mechanisms are those of the industrialized West; his faith in technological "progress" is quasi-religious. Perhaps the most useful aspect of Naipaul's work is his analysis of the role of traditional Hinduism in contemporary India. Naipaul argues that Hinduism (which he never attempts to define and thus appears to view as static and unchanging) has provided the great roadblock to India's "development," the chains of the past are such that Indian civilization is permanently crippled. In this conclusion, Naipaul's cultural biases have intervened to obscure the simple fact that "Hinduism" (and whatever scope of behavior patterns it encompasses at a given point in time) has been able to adapt itself and the culture which it represents in a way that goes far beyond the restrictive Western labeling of it as a "religion." If one views Hinduism merely as a religion, Naipaul's conclusions are powerful and perhaps unavoidable, but a broader approach, conditioned by more malleable perceptions, leads to another view, a view represented by Ved Mehta.

The first question to be asked about Ved Mehta's *Mahatma Gandhi and His Apostles* is "Why another book on Gandhi?" Indeed, if Mehta's book were only a book on Gandhi, it would be easy to relegate it to the great heap of pre-existing literature on the Mahatma and effectively dismiss it. However, Mehta's work (also a compilation of essays, these from the *New Yorker*) touches much more than the personality of Gandhi, for it deals with the more general issue of the evolution and maintenance of a cultural symbol. Beyond the mandatory survey of Gandhi's life, Mehta's book deals not with the Mahatma, but rather with the impact of his message and his image on contemporary India, especially as viewed through the eyes of those who experienced him directly. The "real" Gandhi is as elusive here as he was in South Africa and India in the decades before 1948. Mehta avoids the emotional issue of whether Gandhi's influence was positive or negative; for him it is enough that Gandhi was.

To Naipaul, Gandhi embodied the negative (i.e.,

non-Western) aspects of Indian culture. He views Gandhi as a representative of what he terms India's "trapdoors into a bottomless past," yet another negative point in a negative society where, as Naipaul has a "young foreign academic" observe, people do "their 'potties' on the street." Naipaul, in his obvious revulsion from this face of India, is unable to pass beyond simplistic Freudianism to evaluate the culture. This is by no means to suggest that Naipaul's observations are of no utility. He has produced a stimulating and—as we have come to expect from V. S. Naipaul—well-written set of essays which should be considered seriously by all who participate in the Westerner's "love-hate" relationship with India. Ved Mehta, on the other hand, has given us a sensitive view of India and a personalized experience of the meaning of Mahatma Gandhi. His contribution will be useful for those who want to learn more about Gandhi, but will be even more valuable for those who strive toward an understanding of India.

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K. M. PANIKKAR. *An Autobiography*. Translated by K. KRISHNAMURTHY. New York: Oxford University Press. 1977. Pp. vi, 372. \$12.50.

The illustrious generation of K. M. Panikkar (1894–1963), of highly educated, honest, and urbane administrators who brought India through the trials of independence into modern nationhood, has passed away. India's fate now lies in the uncertain cauldron of politics. Since very few prominent Indians of his time have written their autobiographies, we are particularly fortunate in having Panikkar's, written in Malayalam and smoothly translated into English by K. Krishnamurthy. The work, however, does not reveal the man, but his career. Psychological insights are lacking, and even personal relations are largely omitted. Panikkar would probably have explained that such revelations would not interest serious readers. Like most Indians of his generation, he was inhibited by Victorian reserve.

Also typical of ruling-class Indians was the effect on Panikkar of his four years at Oxford. The experience transformed a parochial South Indian with close ties to his matrilineal family into a cosmopolitan intellectual with strong nationalist convictions. Nationalism for most Indians never prescribed severance of ties with the instruments of British power but rather a grasping of those instruments. Among the various strategies available to achieve power, and one specially suitable to a Travancorean, was service under one of the semi-

autonomous Indian princes. Brief service in the Kashmir state led to much more useful work for Panikkar; he joined the Chamber of Princes as that feudalistic appendage of British rule tried to prepare for the modern age of representative government. Panikkar tells the story of the princes' jealous regard for their glittering royal privileges and their pathetic dependence on the foreigners. He could not convince them to work for an Indian federation under the 1935 act, by which some of their powers might have been saved. Later he refined his statecraft during very successful tours in the states of Bikaner and Patiala as dewan, or prime minister.

As independence approached, Panikkar, foreseeing the end of Indian monarchs, assisted the central authorities under Nehru in taking over the states—probably his greatest service to India. Later he helped reorganize India's administrative units, a politically sensitive job that could only have been carried out peacefully while the professionals still retained control.

Topping off his career were ambassadorial assignments to China, Egypt, and France. His Chinese post, which spanned the Communist takeover, could not be regarded as advantageous to India: he failed to convince Nehru of China's dangers. Nevertheless one of his finest monographs, *Geographical Factors in Indian History* (1955), stresses the threat to India of a militarized Tibet. While in Peking, as during all his working assignments, Panikkar wrote systematically for publication. His biased but stimulating work, *Asia and Western Dominance* (1953), was composed there using the libraries at the British embassy and the Episcopal mission.

If his inner life was ever exposed, it was through his impressive literary efforts—forty-six books, poems, plays, and novels published in English and Malayalam. His *Survey of Indian History* (1947) is a tiny classic and stands up well as part of the much-needed revision of Indian historical writing to reflect an Indian, instead of a British, nationalist outlook.

India was fortunate in having its Panikkar to stabilize the institutions of a new state. They made the transition from dependency to nationhood less hazardous than it has been in many other new states.

CHARLES H. HEIMSATH
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GEORGE WILLIAMS CARRINGTON. *Foreigners in Formosa, 1841-1874*. San Francisco: Chinese Materials Center. 1977. Pp. xiii, 308. \$12.50.

This book offers a tedious account of the British, American, and Japanese in Formosa (Taiwan),

from the time of the Opium War to the Japanese invasion. Despite its title, not all the foreigners in question actually went there; for example, three Americans who, from 1856 to 1857, proposed the annexation of Formosa through either occupation or purchase—Matthew Perry, Townsend Harris, and Peter Parkers—never set foot on the island.

Together with its description of foreign interests on Formosa, the book traces seven spheres of activity and occurrences: shipwrecks, coal mining, annexation, scientific exploration, trading, missionary work, and the Japanese expedition. Ocean-going voyagers were endangered not only by typhoons, destructive sea currents, reefs, and shoals, but also by aborigines who killed the survivors cast ashore. Although the wreck of the American ship *Rover* in 1867 precipitated the military intervention by the U.S. Consul in Amoy, Charles W. Le Gendre, the issue of a policy concerning the rescue of shipwreck survivors remained unanswered. Nevertheless, coal discoveries in the Keelung area spurred Western interest in trade with the island after 1847. Commercial and strategic considerations even prompted some Americans to plot the acquisition of Formosa, but Washington rejected the scheme. After 1860 when the island was officially open to foreigners, several scholarly visitors produced valuable eyewitness reports and more traders came to exchange opium for camphor, tea, sugar, rice, and coal. Commercial contact, however, bred conflict between the avid foreign merchants and intransigent Chinese officials, and in 1868 caused the British to resort to gunboat diplomacy. With traders came missionaries, some more successful than others. The Catholics made only slow progress in preaching the Gospel; in contrast, the Presbyterians, who arrived in 1865, worked harder and converted more people. The book concludes with the 1874 Japanese invasion, which Americans advised and assisted. The expedition, undertaken primarily as an outlet for the discontented samurai, marked Japan's initial drive toward expansion.

Short on analysis, the book offers few original insights. No attempt is made to assess the influence of foreigners on the physical development of the island. George Williams Carrington relies heavily on James Davidson's *The Island of Formosa*, a book published seventy-five years ago, and generally articulates the views of other scholars, such as Leonard Gordon and Sophia Yen. Westerners' reports are summarized by the author often with gratuitous overlap and without question of their historical veracity. That "Formosa was still largely unknown and unexplored in 1860" (p. 107) was only true to Westerners, for the Chinese by that time had inhabited most of its western coast. It is disappointing that the author did not consult any

primary or secondary source in Chinese. A glance at Chinese local gazetteers would have saved him from such mistakes as his assertion that the Hakkas migrated to Formosa before the Southern Fujienese and that by "A.D. 1000 Hakkas controlled the southern half of the west of the island" (p. 17). Chinese primary sources indicate that in 1841, during the Opium War, British warships attacked Keelung twice, a fact that the author omitted. Furthermore, contrary to the position taken by Le Gendre and the Japanese, Chinese documents show that the Tsungli Yamen officials considered Formosa to be Chinese territory and the aborigines their subjects. Despite these limitations, the book is a welcome addition to our knowledge of Western understanding and misunderstanding concerning Formosa during the nineteenth century.

Carrington ends his book with this apt observation: "The foreigners had to learn to value an understanding of a culture very different from their own and a willingness to abide by law, not force, in their relationships with Formosa." It is a historical irony that eleven years after the Japanese incursion, the French invaded the Pescadores and northern Formosa.

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JAMES J. FOX. *Harvest of the Palm: Ecological Change in Eastern Indonesia*. Cambridge: Harvard University Press. 1977. Pp. xv, 290. \$15.00.

Stretching eastward from Java is a chain of islands known as the Lesser Sundas. It is a realm better known to anthropologists than to historians. *Harvest of the Palm* is by an anthropologist who has written the history of two of the smaller islands in the chain, Roti and Savu. Since 1962 James J. Fox has lived in, and conducted research about, these two islands; he has previously written about their social structure and belief patterns.

What immediately sets these two small islands apart from their larger neighbors is the comparative density of their population. They have even managed to settle migrant communities on their nearest largest neighbors—Rotinese on Timor and Savunese on Sumba. At home and abroad the Rotinese and Savunese have managed a more prosperous life than their larger-island neighbors. Since the Rotinese and Savunese are quite dissimilar in self-conception and social structure, the fundamental cause of their success cannot be sought in a distinctive social adaptability. Their crucial similarity, however, lies in their ecological dependence upon a palm-based economy.

The lontar palm (*Borassus sundaicus* Beccari) provides the basic food supply for the Rotinese and Savunese. This nutrition comes in the form of a

sugary liquid. This palm liquid is also used to fatten pigs. Though other crops are grown and consumed, the bountiful nutritional produce of the palm has served as the nexus of the food economy. These islands and their migrant communities on the other islands have developed a denser, better-fed population than is possible under the swidden-based economy of their larger neighbors. The swidden system on Timor and Sumba is presently not even sustaining the less densely populated indigenous sections.

Fox's historical narrative is fascinating. Palm growing was known in both Roti and Savu before the coming of the Europeans. The arrival of the Dutch, though few in number, brought profound changes, first to Roti and somewhat later to Savu. Dutch interference in political affairs was incisive; resistance was futile so adjustment was the route for survival. Christianity and education in the Malay language (the two went together) raised the consciousness of the islanders, exposed them to a wider world, and caused some of the emigration. Induction into new jobs such as military, police, and teaching, starting already under the Dutch East India Company, resulted in a broader and more modern economic base. The initial population density, plus the general barrenness of these little islands (which discouraged plant and livestock experiments conducted on the larger islands), allowed the Rotinese and Savunese to take advantage of the opportunities offered by the Europeans while retaining their traditional economic base around the lontar palm. Today they occupy more important positions in Indonesian affairs than their numbers would seem to warrant.

Especially interesting is the way in which Fox has set about gathering data for constructing his historical narrative. The Rotinese have extensive genealogical records which proved extremely accurate, but these do not weigh heavily in the type of history produced here. More important to Fox's story is the ecological, economic, and social changes of these islands and their people. Information for this is derived from archival records in Jakarta and Holland, from travelers' accounts, from demographic bits and pieces found in a wide variety of official and casual reports, from missionary journals, and from talking and living with the Rotinese and Savunese. The author's close personal experience with the islanders helps to tie together the bits of information obtained from retrospective sources. Few persons will have a specific interest in Roti and Savu. The value of this book, however, is as a model that demonstrates how a social-economic-ecological history can be produced for an area that previously had no written past.

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EVERETT EMERSON. *Puritanism in America, 1620-1750*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1977. Pp. 180. \$8.50.

Few scholars are more familiar with the literature by and about American Puritans than Everett Emerson, editor of the journal *Early American Literature*. His erudition makes this brief introduction to the subject a pleasure to read and his footnotes a scholar's happy hunting ground. The purpose of the book, in the words of the author in the preface, is to provide an "overview of the historical development of Puritanism in the seventeenth and early eighteenth centuries, with particular attention to its ideas, the changes that took place within it, and its cultural contributions."

Predictably the point of view is decidedly literary. Emerson, in incredibly brief compass, attempts to summarize much of the recent work on the social history of colonial New England in order to set the writings of Puritan intellectuals within their evolving cultural context. He is not interested in social, demographic, or political analyses for their own sakes, and, for that reason, the results will not appear wholly satisfactory to most colonial historians. As a general cultural history this book is not as useful as Larzer Ziff's much fuller *Puritanism in America: New Culture in a New World* (1973).

But the author does succeed in his goal of using recent findings and interpretations to illuminate the thought and writings of such notables as Edward Taylor, Anne Bradstreet, Thomas Hooker, and Cotton Mather. His critical appraisals of Puritan literature are stimulating, lucid, and sympathetic. In particular, his two chapters, "Some Puritan Achievements" on prose and poetry and "Glimpses at the Puritan Consciousness" on the diarists Michael Wigglesworth, Samuel Seward, and Thomas Shepard, are superb and should be read most carefully, especially by those of us in colonial history who tend to ignore "traditional" sources in favor of tax lists and court records.

Emerson's description of Puritan literary culture fits within the tradition established by Perry Miller with its emphasis on the relationship of Puritanism "to the development of capitalism and democracy" and to the "American character" (p. 11). He discusses skillfully the themes of introspection, sense of original sin, and special destiny. In fact, the book can be seen as a statement of that tradition set against a summary of recent work by social historians in order to outline, but not really establish, a possible synthesis. To many the two approaches may seem hopelessly antagonistic. In this book they are not, and that is its main contribution.

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J. WILLIAM T. YOUNGS, JR. *God's Messengers: Religious Leadership in Colonial New England, 1700-1750*. Baltimore: Johns Hopkins University Press. 1976. Pp. xi, 176. \$10.00.

The appearance of David Hall's *The Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century* (1971) marked an important step in expanding scholarly knowledge of colonial New England. By interweaving the thought of the clergy with an awareness of their experiences and concerns as members of a professional group, Hall was able to shed new light on colonial ideas and society and, most importantly, on their interaction. Because it takes up the same topic where *The Faithful Shepherd* leaves off, J. William T. Youngs' *God's Messengers: Religious Leadership in Colonial New England, 1700-1750* invites comparison with Hall's study.

The early chapters of *God's Messengers* detail various dimensions of the clerical profession. Education, apprenticeship, ordination, domestic life, and pastoral duties are carefully examined. Youngs' integration of the relevant secondary studies with his own research into primary sources enables him to provide an informative survey of the externals of clerical existence. The latter chapters are devoted to examining the ministers' quest for enhanced status in the eighteenth century. Struggling to achieve the respect which they believed due them, God's messengers expended considerable energy in an attempt to establish a quasi-aristocratic control of the region's churches by strengthening their authority within their individual congregations and by establishing ministerial associations which represented a form of "congregational clericalism." These efforts were opposed by laymen who were more conservative than their pastors in religious matters. By the 1740s the clergy had embarked on a new approach and plunged into revivalism as a means of stimulating attentive congregations and enhancing respect for the ministry.

In charting the progress of change Youngs consistently exaggerates the importance of eighteenth-century developments because he does not bring to his task the broad knowledge of Continental Protestant, English Puritan, and colonial New England thought and practices that so enriches David Hall's study of the seventeenth century. Youngs views the transformation of the Boston Thursday Lecture into a forum for ministers as an innovation on the road to congregational clericalism, yet there were many precedents for such conclaves, including the Elizabethan Puritan prophesyings and the ministerial gatherings held monthly in Thomas Hooker's Chelmsford early in the seventeenth century. The fact that the laity could be more conservative than their pastors is hardly a surprise to those familiar with Patrick Collinson's studies of

Elizabethan Puritanism or with Robert Pope's work on the Half-Way Covenant. The association movement of the early eighteenth century is yet another facet of the story that is but loosely related to its English and colonial antecedents (Youngs seems unaware of Robert Scholz's dissertation and article on clerical consociations); nor is it placed in the context of similar associations among post-Restoration English Congregationalists. Though this movement toward clerical associations forms a key part of Youngs' interpretation, he ignores the existence of a Presbyterian tradition in New England; the differences he finds, however, between Increase Mather and Benjamin Colman are certainly at least partially reflections of Congregational-Presbyterian polity differences.

Because he posits a uniqueness in eighteenth-century developments that they did not necessarily possess, Youngs' explanations for the causes of change are likewise suspect. Though its subject is one of real promise, *God's Messengers* is too narrow in focus to fulfill that promise.

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ROBERT J. TAYLOR *et al.*, editors. *Papers of John Adams*. Volume 1, *September 1755–October 1773*; volume 2, *December 1773–April 1775*. (Series 3, General Correspondence and Other Papers of the Adams Statesmen.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xlviii, 356; xii, 466. \$50.00 the set.

In the *Papers of John Adams* the superb standard of editorial scholarship that has been the hallmark of the Adams papers remains evident. It is all there: scrupulous care in presenting the texts; thorough, judicious, and insightful annotation; and the detailed, analytic system of indexing that makes it possible to consult the published Adams papers so efficiently. Because these volumes, covering September 1755 to April 1775, overlap chronologically with previously published volumes—*Diary and Autobiography*, *Earliest Diary*, *Legal Papers*, and *Adams Family Correspondence*—cross-references are provided systematically. As a result the new volumes interlock closely with the old so as to enhance the utility of each part of the entire group. Here L. H. Butterfield's wisdom in dividing the vast, four-generation collection of documents into three series (diaries, family correspondence, and general correspondence and public papers) is vindicated. The coherence of particular groups of documents is preserved—whether correspondence, newspaper essays, or the drafts of state papers. At the same time cross-references to parallel volumes make it easy to follow the progression of events in Adams' personal and political life.

The editorial challenges presented by the third

series exceed those of the diaries and family letters. Here many authors are represented among Adams' correspondents, tricky questions of attribution exist, and the disconnected character of the documents—ranging from reflective letters to old college friends, to town reports with joint authors, and to political treatises—cannot provide the satisfying literary experience that readers of the *Diary* and *Family Correspondence* have enjoyed. Yet Robert J. Taylor and his colleagues have done a splendid job in overcoming these obstacles. Major groups of papers, like Adams' essays on the independence of the judiciary and his contributions to the Massachusetts House of Representatives' debate with Governor Thomas Hutchinson over the limits of parliamentary supremacy, are introduced with extended headnotes that set the documents in context and explain their connections. Each of these headnotes, and there are over a dozen, is a learned account of the historical issues, Adams' role in them, and the background of the documents. While each note is concise, they are written with a candor and deliberation that enable readers to make their own judgments. The editors have given every reader an immense head start by putting their erudition and insight at our service.

In general, the documents printed in these volumes are not unfamiliar. They begin with Adams' letters as a Worcester schoolmaster in training for the law, go on to cover his emergence as a Whig spokesman, and end on the eve of the battle of Lexington and Concord. Many of them appeared in Charles Francis Adams' edition of his grandfather's works more than a century ago, or in the *Warren-Adams Letters* (Massachusetts Historical Society, *Collections*, Vols. 72, 73), and some, like the *Novanglus* essays (constituting one-third of volume 2) have often been reprinted. Yet because the editorial work opens up the material so effectively, including "new" documents that have been located only in the past three decades and others that had never been printed, the *Papers* will contribute substantially to the reevaluation of Adams that has been underway since the publication of the Adams Papers began with the *Diary and Autobiography* in 1961. Because the ideologue and the political leader were combined in the person of John Adams his papers are especially important for tracing the interaction between experience and ideology in the movement for independence and, as further volumes appear, in the creation of the United States of America.

RICHARD D. BROWN
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JOHN E. FERLING. *The Loyalist Mind: Joseph Galloway and the American Revolution*. University Park: Pennsylvania State University Press. 1977. Pp. 157. \$10.00.

Noting that traditional biographies have produced "stilted, legalistic-sounding, one-dimensional" portraits which offer "speculative inquiries and conjectures" yet fail to grasp the "real man," John E. Ferling examines "a less elusive object, the mind of Joseph Galloway" (p. 2). The book consists of two sections: a concise biographical sketch followed by a series of essays which both examine the Pennsylvanian's views on government, the empire, and the coming of the Revolution and compare his ideas with those of other "articulate" loyalists. Ferling demonstrates that Galloway was a "mainstream" loyalist who supported resistance but rejected revolution, believed that government existed to protect man from himself, regarded the empire as a political extension of the nation, and felt that the Revolution resulted from historical imperatives as well as malevolent machinations. But little credence can be given the claim that only Galloway carried his ideas "to their natural conclusions" and overcame "the pitfall of indecisiveness" by acting decisively to prevent the rebellion (p. 112).

This book, which takes its conceptual cues from Bernard Bailyn, exhibits not only the shortcomings inherent in a narrowly ideational approach to human behavior but also the special problems associated with such a study of Galloway. Because the bulk of Galloway's papers were destroyed in the Revolution, Ferling relies heavily upon a small corpus of published material dating mostly from 1775 or 1779-83; these writings, justificatory tracts rather than disinterested discourses, provide fragile supports for intellectual biography. The result, perhaps unavoidably, is a static study in which ideas generally appear *in vacuo*—an exposition of the political dimension of a mature loyalist "mind" which says little about the origins or the evolution of those ideas.

If the book is necessarily unsatisfying because of source limitations, it is unnecessarily flawed in execution. More significant than the elementary errors, annoying anachronisms, contradictions, and redundancies are the non sequiturs and interpretive inconsistencies. Straining to compensate for documentary inadequacies and to make Galloway an ideologue, Ferling repeatedly explains without really explaining; facile catchwords and bald assertions often appear in lieu of rigorous analysis. (I wonder what Galloway, a cynic who believed man was ruled by self-interest and avarice, would think of this intellectual assessment.) Ferling obviously does not feel entirely comfortable with his analysis of what made Joseph run. Perhaps this frustration explains the largely ahistorical "Conclusion" which unfairly contends that "by any standard of evaluation Joseph Galloway was a failure" (p. 128). I fear that Ferling's critique of previous biographies is applicable to his own work.

Although Ferling apparently has not used the Galloway papers in the Henry E. Huntington Library, he has done about as much with his subject as possible. Fashioning an in-depth analysis of Galloway's political thought is no mean task; thus this monograph makes a substantial contribution to an understanding of the composite "loyalist mind."

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NATHAN O. HATCH. *The Sacred Cause of Liberty: Republican Thought and the Millennium in Revolutionary New England*. New Haven: Yale University Press. 1977. Pp. xi, 197. \$12.50.

This short book summarizes the history of "civil millennialism" from the middle of the eighteenth century until the early nineteenth. In it Nathan O. Hatch helps to open up further the long neglected, recently attractive subject of American millennialism. Since his emphasis is on political applications, he has chosen to read sermons rather than theoretical treatises, and he has read a good many. He has also mastered a large, difficult, and sometimes elusive secondary literature.

In his introduction, citing Butterfield's *Whig Interpretation*, Hatch says that historians dealing with the late eighteenth century have failed to escape their own climate of opinion. This has resulted in an undue separation between religious and political history and a tendency to treat religion exclusively in terms of its political consequences. There is some truth in these charges, but Hatch fails to realize how very difficult it is altogether to escape either a presentist or a secular view of the religious past.

Civil millennialism, Hatch tells us, came into existence shortly before the time of the Seven Years War. It consisted in a coming together of "civic humanism" and Protestant millennial thought and resulted in an almost complete politicizing of an originally religious tradition. In the 1750s, Britain was a part of the Protestant Israel, to be defended against the popish dragon. Only a little later, at the time of the Stamp Act and the fear of bishops, England replaced France as a target of millennial rhetoric, and during the Revolution American victory was expected to usher in the new day. In the 1780s, the clergy chiefly emphasized moral dangers at home, insisting on the necessity of virtue for republics. This linking of morality, liberty, and religion, carried forward into the 1790s, explains how ministers who had been libertarians in 1776 became "fearful traditionalists" when they confronted the French Revolution. The early nineteenth-century combination of exu-

berant republicanism and millennialism is to be understood, as it has not been, as a further development of this eighteenth-century blending of Christian and republican themes.

It is no indictment of Hatch to say that his swift narrative skips over two almost universally neglected periods, the early eighteenth century and the first quarter of the nineteenth. My more serious criticism is that he tends to exaggerate the innovative and changing elements in the tradition he is discussing, and to slight what is continuous. Civil millennialism was, he says, "the first substantively new eschatology since the Reformation." As he describes it, however, it is the applications rather than the eschatological substance that is new. At his best, especially in his later chapters, Hatch helpfully calls attention to the continuing tradition of New England politics and religion. At times, however, he injures his case by overdramatizing the changes in this tradition. Of course the emphasis changed as New England's enemies changed from French to British kings, and then to corruptionists, Jacobins, and Jeffersonians. What still needs further illumination, however, is the essential content of the ideology that lasted through this period—and I suspect, before and after—despite shifting emphases on its various components. Order, frugality, morality, and liberty were all seen as bulwarks of pure religion and none was likely to be entirely forgotten. More than Hatch always explains, hopes and fears, chastening trials and millennial promises, were intermingled.

At times, particularly in his later chapters, Hatch is oddly indignant that the New England clergy were at the same time "contemporary" in their republicanism and "reactionary" (a word used repeatedly) in their assumptions about human character. With this vocabulary, does not the Whig interpretation raise its battered but durable head?

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DOROTHY ANN LIPSON. *Freemasonry in Federalist Connecticut, 1789–1835*. Princeton: Princeton University Press. 1977. Pp. x, 380. \$18.50.

Dorothy Ann Lipson has chosen a title so modest as to be misleading. This sharply focused but broadly based study tells us as much about the society of New England—and of New York, Pennsylvania, and Ohio, to which so many of its inhabitants migrated—as have many of the super-numerary recent studies of the decline and fall of

Calvinism. The focus is Freemasonry in Connecticut, but the subject is the conflict of sectarian religions with a secular religion.

Historians have often studied anti-Masonry because it had political as well as social manifestations. David Davis, Michael Holt, Seymour Lipset and Earl Raab, and Ronald Formisano have done good studies of that phenomenon, especially within the intellectual context of McCarthyite, paranoid political styles. Freemasonry, however, has not been at all well studied by modern professional historians. Lipson takes Masonic historiography a long way toward full investigation, and her work will have to be the starting place for any professional cultivating the field.

Since Freemasonry has been given so little scholarly attention, Lipson has to do a good deal of foundation work before constructing her conceptual edifice. In the context of a Protestant Christian and liberal democratic social structure, an institution that is neither religious nor political but commands a primary loyalty is hard to define. Lipson describes Freemasonry as a dissent to the Standing Order (p. 103), a counterestablishment (p. 8), a counterculture (p. 228), and a supra-religious, supradenominational, supranational fraternity (pp. 175, 246, 323). Freemasonry was also an alternative subcommunity (p. 151), an "autonomous social group," "defined by selective affiliation" (pp. 199–200, 226), whose members adhered to "a subsystem of ideas and values at odds with the Calvinist tradition" (p. 228). Freemasons attempted to define, teach, and enforce a secular moralism that was Christian and anti-non-Christian but refused to distribute Bibles because they saw that as a sectarian activity. In practice, Freemasons eschewed ecumenicalism and interdenominationalism and claimed to profess and practice a universal morality which was in fact Christian (they called it catholic).

The morality professed was one of "virtue and charity, masonically defined" (p. 187), which seems to mean that all members were to be "good men and true," who owed allegiance to each other and to the order, first, and to other elements of society such as church, state, and even wives, thereafter. It was not an allegiance that could parallel the others, the way, say, that empirical science and fundamental Christianity so often did in the late nineteenth century. It was more like George Kennan's world Communism, a fluid element that was always expanding into areas of least resistance, stopping only in the face of resolute opposition.

Such opposition was attempted in the religious area by the Protestant churches and in the political area by the anti-Masonic movement, but both were unavailing against what was neither religious

nor political. Lipson sees Freemasonry as a social movement (with economic elements such as business, insurance, and charity functions) whose effective antagonists were family—especially wives—and social disapproval. Thus her approach to the subject is to locate it within the liberal Protestant ethos at a time when that ethos was undergoing fundamental changes. For those who have concluded that nothing new can be said about the decline of Calvinism in New England, *Freemasonry in Federalist Connecticut* will be a revelation. In describing this counterculture, Lipson illuminates by juxtaposition of opposites (secular-religious, exclusivist-universal, elitist-egalitarian, secret-public) much about New England Protestantism at a point when the evangelical thrust was toppling rational Calvinism for the final time. This aspect of her work is best represented by a chapter called “The Dynamics of Masonic Dissent”: a section of this chapter brilliantly describes the divergent uses of funerals by churches and the Masons.

Freemasonry filled a need for widely dispersed social, political, and business connections in a new nation; for a sense of place in a fearfully dynamic social hierarchy; and for a practical moral code in a rapidly secularizing world with sectarian claims of a bewildering variety. It appealed to all sorts of men, rich and poor, high and low, of whatever denomination, level of education or occupation. It was dominated by the same kinds of men who dominated town and state politics and business, and the commercially oriented were much over-represented—though perhaps because membership was expensive. Lipson’s text and appendix charts show that membership represented a cross section of male society; she is less successful in describing what it was that brought some men and not others to the Order. The best she can do is to tell us that “membership seems best explained as an association of like-minded men who were attracted to Masonry because of its exotic qualities and its personal or social usefulness” (p. 238).

A thorough grounding in all sorts of social science literature and an application of “counting techniques” inform *Freemasonry in Federalist Connecticut*, but Lipson is primarily an intellectual historian who most successfully makes the connections between ideas and their social context. That this review is written without a single reference to Connecticut is a much better indication of the scope of the book than is Lipson’s unnecessarily modest and restrictive title. It is not a study of Connecticut; it is instead a brilliantly illuminating depiction of northern United States social attitudes on the eve of modernization.

CHRISTOPHER COLLIER
University of Bridgeport

PATRICK T. CONLEY. *Democracy in Decline: Rhode Island's Constitutional Development, 1776–1841*. Providence: Rhode Island Historical Society. 1977. Pp. xvi, 433. \$13.95.

Patrick T. Conley surveys the constitutional and political history of Rhode Island from the establishment of state government to the eve of the Dorr War in 1840. Then, in an epilogue which is the longest chapter in the book, he attempts to synthesize the recent scholarship on the Dorr War itself. The thesis of this book is suggested in its title: what began as a lively experiment in democracy degenerated over some two centuries into an exclusivist regime which only a near-revolution could alter. While population growth and economic development centered largely in the urbanizing northeast after 1787, the constitutional arrangements of the seventeenth century, unchanged until after 1842, allowed the stable or declining communities of the rural and small-town south and west to maintain control. Using tabular methods and state reports on the census, taxation, and related matters, Conley confirms the findings of earlier scholars that Rhode Island government bore little resemblance to popular democracy by the 1830s.

Conley’s thesis of “Democracy in Decline” succeeds only marginally in explaining the developments in Rhode Island. After all, political ideas change over time, as all things do. Democracy meant something quite different in 1840 than it had in 1640. What worked for the fairly homogeneous and relatively stable communities of the seventeenth century certainly would not for the heterogeneous and rapidly modernizing towns and cities of the nineteenth century. Modernism brought with it important changes in political attitudes and deep social concerns about cherished ways of life. In fairness, Conley understands that these changes occurred. His significant conclusion that ethnic animosities produced the explosive milieu for the Dorr War makes that very clear. And yet, the thesis of decline fails to take into account the emergence of new forms of political culture as conditions changed.

Using the periodic efforts to remodel the basic instrument of government in Rhode Island as the critical moments deserving careful analysis, Conley provides a solid foundation for understanding the events of the 1840s. Moreover, his sensitivity to the ethnic factors in the political culture of the nineteenth century adds a dimension not fully recognized by earlier scholars. The pragmatic character of the early constitutional movements indicates that most people were unaware of the qualitative changes occurring in their lives. The ideas that moved people to action in the 1840s were certainly not new, and Conley quotes extensively from the

speeches, letters, and writings of early reformers to show just how commonplace these ideas were. Nonetheless, these ideas took on new meaning in the context of nativism, political and social reformism, and the socioeconomic conditions of the early 1840s. To the leaders of the suffrage movement, these ideas linked Americans of the fourth generation to the Founding Fathers, imposing a heavy responsibility to preserve the traditional republic. Despite ethnic and socioeconomic complications, only ideological considerations suffice to explain why the suffrage movement differed radically from its predecessors.

Conley's book warrants careful reading by scholars and laymen alike. For it deals with ideas and issues central to the history of American civilization. Whether one agrees or disagrees matters far less than that the dialogue continues in the search for the "real" America.

GEORGE M. DENNISON
Colorado State University

CLYDE N. WILSON and W. EDWIN HEMPHILL, editors.
The Papers of John C. Calhoun. Volume 10, 1825-1829. Columbia: University of South Carolina Press for the South Carolina Department of Archives and History. 1977. Pp. xlvii, 592. \$27.50.

Writing to John A. Dix of Cooperstown, New York, on September 1, 1828, John C. Calhoun committed what may have been one of his most revealing slips of the pen. He wrote that although southerners were certain to be injured greatly by the higher schedule of tariffs, they would remain loyal to the Union, for "nothing can alienate them, but a deliberate conviction, that it is the intention of a majority of our brethren to use the general powers of the Union conferred for the benefit [*sic*] of all the parts, or rather powers which they have usurped from the reserved rights of the State[s] for their own benefit, without regard to the ruinous effects on the South" (p. 414). But with barely a pause Calhoun had trimmed his language from that of nationalist to that of sectionalist. Perhaps his conversion to states' rights was more a change of arguments than ideas. In any case, those who would maintain that the Tariff of Abominations prompted Calhoun's retreat from nationalism will want to read very closely his papers for the period 1825-29.

In a highly cogent and provocative introduction the volume's principal editor, Clyde N. Wilson, re-examines the basic constructs of early nineteenth-century republican thought in America. He suggests that suspicion, restriction, and fragmentation of governmental power formed an integral part of the American folk tradition. Admittedly, ex-

igencies of war and fevers of speculation sometimes displaced popular fear of government, but such distractions seldom lasted more than a few years. Wilson plays down the extent of Calhoun's early flight from Jeffersonian orthodoxy and thereby reduces the distance of the prodigal's return. Vice-president Calhoun staked out his new line of political argumentation as early as 1826. Writing under the pseudonym "Onslow," he tried to repel charges of partisanship made by "Patrick Henry," whom some would identify as John Q. Adams. In defending a strict construction of his powers as the Senate's presiding officer, Calhoun drew heavily on those antipower arguments long ignored in his prior political thinking. The editor clearly traces that rather fancy reasoning by which Calhoun distinguished between a revenue and protective tariff, between a national and sectional tariff, and between a Calhoun and Clay tariff. Wilson's handling of the Onslow-Henry debate and South Carolina's Exposition demonstrates great ability in the field of intellectual history; all who study Calhoun's ideological migrations will want to get acquainted with the tenth volume's introductory essay.

Other significant topics treated in this installment include Calhoun's denunciation of the "corrupt bargain," a reprise of the Elijah Mix scandal, preliminary skirmishing with Jackson's lieutenants over who said what in cabinet debates on the Seminole War, Democratic Party organization, the 1828 presidential election, and numerous private family concerns. A judicious selection of documents, coupled with a consistent application of editorial method, makes this volume one of the most interesting and useful in the series.

WAYNE CUTLER
Vanderbilt University

JOHN M. BELOHLAVEK. *George Mifflin Dallas: Jacksonian Patrician*. University Park: Pennsylvania State University Press. 1977. Pp. xii, 233. \$12.75.

In writing this first published biography of George M. Dallas, John M. Belohlavek does not yield to the temptation to represent his subject as more important than he actually was. Recognizing that James K. Polk's vice-president lacked popular appeal, political dexterity, and personal drive, he argues that he is worthy of study because "his views, ideals, and fears were so average and typical that he mirrored the American mind with perhaps greater fidelity than a Daniel Webster or a Henry Clay, and the other giants of mid-nineteenth-century America."

Yet Belohlavek hardly considers Dallas, who held office under every Democratic president from

Andrew Jackson to James Buchanan, to be a typical leader of that party during the Middle Period. On the contrary, he believes that Dallas—whom he calls a “silk-stocking Jeffersonian in an age of egalitarianism”—was never at home in the Jackson movement. But from the time the Philadelphian deserted John C. Calhoun to climb aboard what is called Jackson’s “captious bandwagon” in 1824, he apparently never seriously considered deserting his party, even though he often disagreed with its programs and generally had little respect for its presidents.

It is difficult to ascertain Belohlavek’s attitude toward the nature of the Jacksonian movement and its leadership. He does not make it clear why he maintains that if Dallas “had been born thirty years earlier he would have been first a Federalist and later a Whig, rather than a Jeffersonian who became a Jacksonian.” While he states that the Quaker City aristocrat had “nothing in common with the mechanics, small farmers, and pioneers in coonskin caps who called themselves supporters of ‘Old Hickory,’ ” he notes that Buchanan, his arch-rival in the Pennsylvania Democracy (and a former Federalist who became a Jacksonian), was “as much the patrician or gentleman in politics as Dallas.”

George M. Dallas: Jacksonian Patrician, despite an occasional awkward sentence, is generally well written, and its publication probably obviates the need for another biography in the near future—unless the missing diaries and letter-books that Dallas is known to have kept throughout his public career should come to light. We have such materials for his diplomatic missions to St. Petersburg and London, and Belohlavek’s biography is at its best in dealing with those aspects of his career.

EDWIN A. MILES
University of Houston

CHARLES L. TODD and ROBERT SONKIN. *Alexander Bryan Johnson: Philosophical Banker*. Syracuse: Syracuse University Press. 1977. Pp. xix, 362. \$16.00.

On September 9, 1967, a century after Alexander Bryan Johnson’s death, a group of semanticists, philosophers, historians, and economists gathered in Utica to celebrate the life and works of this largely forgotten banker and thinker. The proceedings, entitled *Language and Value*, recorded his contributions especially in the field of linguistics in which he anticipated concepts later formulated by Alfred North Whitehead, Ludwig Wittgenstein, and other scholars. Additional details are presented in this well-balanced biography of a multifaceted man whose mind, like a seismograph, reg-

istered many ideological tremors in the national period.

Charles Todd and Robert Sonkin have made good use of Johnson’s autobiography and family letters as well as the literature on reforms and politics. A snob, who advocated marrying for wealth and status, Johnson took as his first wife Abigail Adams, granddaughter of President John Adams. He corresponded regularly with John and John Quincy Adams, and their letters, reproduced at length, are fascinating evidence of their wide-ranging interests and forthright opinions.

Born in England of rabbinical lineage, a fact which he concealed throughout his life, Johnson in 1801 joined his father on the upper Mohawk frontier. After making a fortune buying and selling wheat and potash, the Johnsons branched out into real estate and banking. Alexander became head of the Utica branch of the Ontario Bank of Canadaigua, a position which he held for over thirty years. During the struggle over the Second Bank of the United States, Johnson crusaded against Biddle’s privileges. Although Johnson could not stomach Van Buren’s plan for an independent treasury, he subsequently returned to the Democratic Party. He approved Texas annexation, defended the Fugitive Slave Law, and upheld states’ rights, even the right of secession.

Claiming that “I write to discover what I ought to know,” Johnson authored ten books, scores of articles, and hundreds of letters to the press. His primary interest was in semantics but to his chagrin his lectures and books on this subject evoked practically no response. He would be pleased to know that his early (1826) masterpiece, *A Treatise on Language*, is now available in paperback.

The authors have produced a judicious assessment of an important figure in American economic and intellectual history. A few slips in dates and facts do not mar the integrity of their work. Some readers may regret the absence of footnotes but the quotations are clearly identified. Twelve illustrations and a graceful style add distinction to this significant work.

DAVID MALDWYN ELLIS
Hamilton College

HERBERT WEAVER and WAYNE CUTLER, editors. *Correspondence of James K. Polk*. Volume 4, 1837–1838. Nashville: Vanderbilt University Press. 1977. Pp. xxxvi, 692. \$25.00.

The two years of James K. Polk’s career covered in this volume were a period of transition of power from Andrew Jackson to the new president, Martin Van Buren, and a time of trouble for the new administration. Returning to the Hermitage in the

spring of 1837, the Old Hero maintained his interest and sizeable influence in politics; and as these letters show, his dependence upon his loyal friend, Speaker Polk, increased as a consequence of his situation in retirement. Nothing previously unknown appears in the items included here on this subject, but it takes more understandable shape in the context of all the other materials, chiefly incoming correspondence in the Polk Papers. Jackson and Polk assessed the political scene in Tennessee and the nation and reacted to problems of party organization and the onset of economic depression similarly. The former president relied upon Polk to help lead the Democracy in a direction upon which they perfectly agreed. By August 1838 this involved Polk's decision to run for governor as a way of strengthening the party's position.

If the extant correspondence is an accurate indicator, Polk's world was overwhelmingly that of the politician, interested in the machinery of the party down to the town and county level, the necessity of stump speaking, the indispensability of supportive newspapers (difficulties concerning the Nashville *Union* persisted), and of course the maintenance of partisan enthusiasm. So his activity seemed dedicated to unceasing electioneering. Decidedly more opportunistic than reflective, he dealt with few issues, the main one being the desired independent treasury. He loosely characterized the opposition, personified by John Bell, Hugh White, and to a lesser extent Henry Clay, as little more than latter-day Federalists—hardly an accurate judgment.

One gets only a few glimpses of the elusive inner qualities of the man: a bit on his guardianship of younger brother Sam, something on his interest in his Mississippi plantation, nothing on his relationship to his wife, and also nothing on his non-political mind. A reason for this imperfect image was his personal reserve, but a better one was his endless preoccupation with things political.

With this volume, the editorship of Herbert Weaver closes after twenty years of highly competent work, for which historians will be grateful. His successor is Wayne Cutler, formerly of the Clay Papers, who helped with this volume and will take charge of subsequent ones. With a foundation so well laid, Cutler will be able to continue the project effectively.

MAURICE G. BAXTER
Indiana University,
Bloomington

ROBERT F. SAYRE. *Thoreau and the American Indians*. Princeton: Princeton University Press. 1977. Pp xix, 239. \$14.50.

The legacy of Thoreau's fascination with America's original inhabitants is contained in over a half million words in his unpublished Indian notebooks, in letters and journals, and in major prose works—*A Week on the Concord and Merrimack Rivers*, *Walden*, and *The Maine Woods*. Thoreau is often held up as a rare example of a nineteenth-century white American unbiased toward Indians. To think that is to think individuals possess the ability to step outside their time or culture. Robert F. Sayre explains, using Roy Harvey Pearce's typology, savagism and civilization, that savagism was the cultural paradigm that biased white Americans toward red Americans. Whether they hated or idealized Indians, Thoreau and his contemporaries were savagists. Only after a lifetime of study was Thoreau able to work through some of his cultural prejudices.

Sayre's book is thoughtfully conceptualized and well researched. It exemplifies the best sort of cultural history, for its implications extend beyond Thoreau. If the free-spirited and critical mind of Thoreau, who strove to avoid unthinking endorsement of cultural norms and values, was unable to fully break through the savagist paradigm, what hope was there for ordinary white Americans?

Sayre documents the discrepancy between Thoreau's knowledge of Indians and their actuality. It was "the Indian" who cast his spell upon Thoreau, the solitary hunter in his wilderness domain, not Indians in the collectivity of their social organization. Nonetheless, Thoreau was a dedicated student of Indians and, for his time, a good one. If at the end of his life he retained some savagist prejudices, he outgrew others, particularly as a result of three trips into the Maine wilds with Indian guides. During his final trip in 1857, he came to know and admire his guide, Joe Polis, describing him in *The Maine Woods* with a depth and clarity unsurpassed in the century's literature about Indians. Where Polis' woodcraft might reinforce the paradigm, his enthusiasm for white education, real estate entrepreneurship, the large white house in Oldtown (the one with blinds), and tribal sociality all worked to the contrary. Sayre is somewhat equivocal about whether or not Thoreau overcame the savagist paradigm in his last years, stating at different times that he did, almost did, and did not. His own evidence suggests that, with Polis as guide, Thoreau journeyed nearly through the paradigm's mythos but not beyond.

Previous scholarship, citing the Indian notebooks as evidence, assumed that Thoreau intended to write a book about Indians. Sayre dissents, arguing that while the notebooks demonstrate his serious regard for Indians, there is no hard evidence he planned such a book. Moreover, the be-

lief that he did plan such a book has distracted scholars from careful consideration of what he said about Indians in the books he actually wrote. Sayre's own scholarship admirably makes up for past neglect. His speculation about the unwritten book on Indians, however, opens a controversy without resolving it.

ALLAN M. AXELRAD
California State University,
Fullerton

GARY B. MILLS. *The Forgotten People: Cane River's Creoles of Color*. Baton Rouge: Louisiana State University Press. 1977. Pp. xxx, 277. Cloth \$20.00, paper \$7.95.

Gary B. Mills presents us with a study of a population of *gens de couleur libre*, who have lived in northwest Louisiana since the late eighteenth century and who enjoyed an unusual degree of wealth and prominence prior to the Civil War. Known as the Cane River Creoles, these people have been the subject of literary treatment in Lyle Saxon's *Children of Strangers* (1937) and sociological investigation in Sister Frances Jerome Wood's *Marginality and Identity* (1972). Mills sets out to add a historical perspective to our knowledge of this group.

Comparing oral tradition to civil and ecclesiastical records from the colonial and territorial periods, Mills examines the origins of the Cane River Creoles in history and legend. Despite some inaccuracy in details, he finds the oral tradition to be remarkably faithful to the historical facts. The colony can be traced to Marie Thereze, also known by the African name Coincoin, who was born to slaves of the commandant at Natchitoches. As a young woman she became the concubine of a Frenchman named Pierre Metoyer, who bought her, manumitted her, but never married her. When their alliance ended, Metoyer granted Marie Thereze an annuity and land, and she purchased those of her children who were still slaves. Inter-marriage between her children, Indians, and other free people of color resulted in the formation of a little colony at Isle Brevelle, later known as Cane River.

Mills notes that free people of color in Louisiana had rights not shared by free blacks in other states, including the right to bring suits against whites in court. This contributed to their growth in wealth and status, until they owned extensive cotton lands, including Melrose Plantation, and even slaves. They adopted the language, culture, and religion of the French upper class. But the Civil War caused their financial ruin, and the political restrictions on nonwhites that followed Reconstruction prevented them from regaining their former status.

This book is thoroughly researched and carefully reasoned, and it reflects a close analysis of primary sources. Its importance lies in the fact that it is a challenge to the "two-caste" model of American race relations. Mills asserts that previous studies of *gens de couleur libre* in Louisiana "fail to show distinctly the caste barriers which stood between the slave, the free Negro, and the free men of color in Creole society" (p. xv). But Mills goes on to maintain that the three-caste system was "peculiar" to Louisiana. The wealth, status, and political rights enjoyed by the Cane River Creoles were indeed unusual, but historians continue to overlook other racially mixed populations of intermediate caste status, living throughout the eastern United States before the Civil War.

DAVID STEVEN COHEN
Rutgers University,
Newark

RAY A. BILLINGTON. *America's Frontier Culture: Three Essays*. (Essays on the American West, number 3.) College Station: Texas A & M University Press. 1977. Pp. 97. \$5.00.

Ray Allen Billington has so often been called the leading Turnerian of our day that the phrase has begun to sound like an epitaph for the frontier thesis: after Billington, nothing. The three essays collected in *America's Frontier Culture* were first published in 1954, 1964, and 1975. Thus they mirror Billington's changing strategies in his defense of the Turner thesis over a twenty-year period and are perhaps intended to rebut the notion that his views have been monolithically consistent. One can only speculate on this point since W. Turrentine Jackson's introduction praises the essays more for their stylistic grace than their content and does not tell us *why* they were selected, and Billington himself has chosen to let them stand as written, without comment or amendment.

If *America's Frontier Culture* is intended as a homage to Billington, it may seem ungenerous to quibble over works that are frankly submitted as artifacts in one historian's evolution. But the 1954 essay, "The American Frontiersman," seems too lightweight to merit reprinting. First delivered as a lecture at Oxford, it posits the mountain man as the typical frontiersman writ large, then indulges in a series of wild and woolly anecdotes that must have delighted Billington's audience of English academics nurtured on Buffalo Bill and his ilk. But

no one, including Billington, could still take the essay seriously, either as an accurate depiction of the mountain man or as a defense of Turner's environmentalism, and it must be added that it now seems distressingly ethnocentric in its allusions to mountain men sinking into savagery "even below the level of the Indian" (p. 29). The succeeding essays are better. "The Frontier and American Culture" (1964) typifies the argumentative process that Billington employed most effectively in *America's Frontier Heritage* (1966). Turner's critics are granted everything but ultimate victory. In this essay, for example, Billington amasses evidence to support the contention that traditional culture moved westward with the frontier—even mountain men enjoyed books, he allows—but in the last few pages, having disarmed his reader, he does an abrupt about-face, reversing the flow of the argument by challenging the notion that eastern culture was actually transplanted intact and advancing an impressionistic case for a modified Turnerianism. The most recent essay, "Cowboys, Indians, and the Land of Promise," explores the contrasting images of the West that have entrenched themselves in the European mind over the last two centuries: the Wild West and the New World Eden. The former, with its assumptions about cultural degeneration, can be linked to the eighteenth-century controversy over America's prospects—particularly to the philosophical premise that the New World environment was deleterious—and perhaps even to what Howard Mumford Jones has called the anti-image of America popularized in the sixteenth century, though Billington does not probe such antecedents here. He does relate the Edenic image to the concepts of material plenty, egalitarianism, and political democracy and implies that it may well have played an unrecognized part in European political reform in the late nineteenth and early twentieth centuries.

This attractive, slim volume holds no surprises for the students of Western American history, but it does delineate the path taken by a major figure in the field.

BRIAN W. DIPP
University of Victoria

JOHN A. JAKLE. *Images of the Ohio Valley: A Historical Geography of Travel, 1740 to 1860*. (The Andrew H. Clark Series in the Historical Geography of North America.) New York: Oxford University Press. 1977. Pp. viii, 217. Cloth \$12.50, paper \$6.00.

This is a book of modest length and limited purposes. It describes the landscape of the Ohio Valley between 1740 and 1860 as seen and reported by

early travelers, the great majority of whom were Americans or Britons. The book "deals with the early Ohio Valley landscape, not so much as it was, but more as it was thought to be" (p. vii).

John A. Jakle, who teaches geography at the University of Illinois, proceeds systematically. His first two chapters deal with the travelers themselves. He then moves on to conceptions of the wilderness, of aboriginal life, and of military life during the Indian wars. He concludes with some comparisons between agrarian and urban landscapes. The reader can trace a physical evolution during these years. It moves from an unspoiled landscape to a farming environment and onward to the industrial stage. Overall, the author decides: "The Ohio Valley was considered a region of opportunity and adventure. It was the nation's first 'West.' There the pattern of warfare and treaty violation that deprived the Indians of an entire continent first evolved. There the nation's dominant systems of land tenure and farming were perfected. There the first industrial metropolises of the nation were built" (p. 160).

Accepting the author's objectives in writing this book, one must acknowledge that a creditable job has been done. Jakle's reading of travel materials is thorough and his bibliography is helpful. The prose is efficient, though slowed by some jargon; the countryside does not come to life. What the prose lacks is recovered somewhat by many fine illustrations, charts, and maps. Oxford University Press has made an attractive book and the author has turned up many interesting and vivid details which enlarge our visual perceptions of the Ohio Valley during an important period.

The chief difficulty lies with the concept of the book itself. There is a contrived quality about its subject. Viewing this work as part of Oxford's Historical Geography of North America series one can imagine an editorial effort at manufacturing a topic to fit the series. Though the author tries strenuously to give the book a life of its own, the effort is not wholly persuasive. What is the point of a landscape depicted "not so much as it was, but more as it was thought to be"? Just how accurate were the descriptions? What can we learn about the actual history of this region from these materials? Travel literature, more than most other sources, must be carefully weighed against other kinds of materials if it is to be useful. The author does too little of this, thus leaving unresolved the sense of artificiality which this topic engenders. Readers who find themselves drawn to this topic will, however, come upon a well-researched and intelligently organized monograph.

RICHARD L. RAPSON
*University of Hawaii,
Manoa*

MELVIN G. HOLLI and PETER D'A. JONES, editors. *The Ethnic Frontier: Essays in the History of Group Survival in Chicago and the Midwest*. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1977. Pp. 422. Cloth \$11.95, paper \$7.95.

The Ethnic Frontier is a collection of nine original essays dealing with the interaction between ethnic diversity and the frontier experience. Individually, each author has done an exemplary job of presenting a detailed study of a particular ethnic group's values, institutions, and lifestyles and its interaction with the Midwestern environment. Collectively, these well-organized and cogently argued essays graphically illustrate the complexity of the adjustment process and the essentially simplistic nature of the dichotomy between assimilation and pluralism as mutually exclusive models.

The first three essays deal with the devastating impact of Yankee culture and technology upon less "progressive" ethnic groups. Jacqueline Peterson paints a sensitive and largely sympathetic portrait of the free and easy society of "wild" Chicago, a multiracial mosaic of Indians, Frenchmen, Anglos, métis, and other mixed bloods who evinced little interest in building "a city that works." Present- and pleasure-oriented, this motley crew was helpless before the onslaught of rapacious Yankees willing to postpone gratification in anticipation of future profit. Melvin G. Holli delineates much the same conflict between Yankee culture and the archaic "seigneurial" system of the established French "*habitants*" in early Detroit. Overrun by the modern Yankee economic system and possessing no real political acumen, the *habitants* were quickly absorbed or shunted aside, asserting their differing value systems only in periodic struggles over temperance, Sabbatarianism, antislavery, or free public education. Hugo P. Leaming focuses on a "new ethnic group," the Ben Ishmalites, an amalgamation of Indians, blacks, and poor whites whose religion and customs served as a bridge between African and American Islam. The cultural conflict between this nomadic hunter-fisher people and Yankee commercial society led to the 1907 Indiana Plan for the sterilization and dispersment of the Ben Ishmalites.

The remaining essays concern themselves primarily with the survival strategies of specific ethnic groups on the urban frontier, especially in Chicago. Victor Greene analyzes the efforts of a variety of "traditional progressives"—Swedish ministers, Polish priests, Italian padrone-bankers, and Jewish newspaper editors in various cities—to serve as cultural brokers between their communities and established society. As such, they shaped their ethnic communities into open-ended "sanctuaries," in which their fellow nationals

could find cultural succor and from which they could securely advance up the socioeconomic scale.

Edward Kantowicz concentrates upon Chicago's Poles and their "survival through solidarity" strategy which emphasized separate development, institutional completeness, and recognition politics at the expense of influence in city-wide affairs. By contrast, as Charles Branham carefully details, Chicago's blacks diligently worked for integration in the years before the Great Migration until the militancy of race politics and the civil protest tradition gave way to a black political machine run by professional politicians largely content to compete for rewards as the clients of white patrons. For the city's Jews, according to Edward Mazur, the most bitter conflicts were intramural, pitting the established Germans against the working class, Yiddish, and orthodox Eastern Europeans, before Henry Horner emerged as a political symbol around which both groups could rally. Internal divisions, this time between pre- and post-World War I arrivals, also rent Chicago's Chicano community as Louise Anõ Nueve Kerr skillfully illustrates, retarding the assimilationist vision of the earlier migrants, and dooming the efforts of agencies trying to promote integration. In the final selection, Arnold Hirsch examines two decades of conflict over housing between blacks and various white ethnic groups in Chicago. Hirsch views each riot as a "grim parody of a community sing" (p. 20), in which women, the elderly, and children engaged in purposeful destruction designed to protect residential "turf."

These nine essays naturally vary somewhat in effectiveness and significance, but the overall quality is considerably higher and more consistent than is generally found in such collaborative efforts. Melvin G. Holli and Peter d'A. Jones have done an outstanding job of delineating a highly important theme and of eliciting essays that effectively explore important aspects of the whole. They have helped to push back "the ethnic frontier" a significant distance.

JOHN D. BUENKER
University of Wisconsin—
Parkside

JOSEPH KASTNER. *A Species of Eternity*. New York: Alfred A. Knopf. 1977. Pp. xiv, 350. \$15.00.

This is a charming book, put together with a fine eye for texture and fabric. It is a panorama of early American natural history, told in terms of field naturalists from Colden and Bartram to Maclure and Say, but offering a second ending too—or a

new beginning—in the “closet naturalists,” Torrey and Gray.

Joseph Kastner steps deftly from biographical stone to stone, giving some sense of relationships and sequence, but without any real concern for networks or institutions, for interpretation or scientific evaluation, or for social meaning or historical movement. His strength is in narrative, drama, and anecdote. He writes well and, wherever possible, he lets his travelers speak for themselves, often in long passages. Fortunately, the literary images of William Bartram, the pungent prose of his father John, and the remarkable stories of Thomas Nuttall are worthy of this celebration.

The book is written from the published record and is itself a commentary on the number of scholarly studies and reprinted sources that have appeared in this field in recent decades. Although the text is not documented, chapter bibliographies make it relatively easy to follow the trail. Kastner is familiar with this material, which he has applied to his own purpose—the telling of a fascinating story, not the development of new understanding.

While often recognizing interpretations that have been placed upon actions and writings of the naturalists, Kastner has a way of turning them aside when they intrude upon the fabric of the dialogue he relates. For example, he acknowledges the criticisms of Cadwallader Colden as an administrator and especially as author of *The Principles of Action in Matter* but acts as if they represented merely differences of opinion. He writes often of the primacy of Philadelphia in natural history—but most often to speak of its relative decline. Moreover, by emphasizing Colden, Samuel Latham Mitchill, and David Hosack an unusual importance for New York is suggested—but never addressed or examined.

Occasionally Kastner's eye is off, as in the consistent misspelling of Sacajawea, but the worst image failure is not his fault. The book is heavily illustrated with black and white and with two packets of color plates. These are well selected and they are more than illustrations; they are an integral part of the story. Unfortunately, the reproductions are so bad that they cannot serve their purpose. All the subtlety and detail of the color is lost and the black and whites are generally muddy.

The book, however, is successful in conveying the flavor and excitement of an important enterprise. It should reach out to general readers and to historians not at home in this field.

BROOKE HINDLE
Smithsonian Institution

AUBREY L. HAINES. *The Yellowstone Story: A History of Our First National Park*. In two volumes. Boulder:

Colorado Associated University Press. 1977. Pp. xx, 385; xv, 543. Cloth \$10.00 each; paper \$3.95 each.

This lengthy study traces the history of the Yellowstone region from the time when hunters and gatherers migrated into the Rocky Mountains thousands of years ago to the post-World War II era when hordes of tourists inundated Yellowstone National Park. Aubrey Haines pursues his stated purpose—the “re-creation of enough of Yellowstone's past to typify the whole of it”—through extensive description of the activities of those people whom he believes contributed significantly either to the actual discovery and development of the region or to the stories and legends that surround the park's history.

Haines, former park historian at Yellowstone National Park, divides each of his two volumes into two sections: the first volume includes the prepark period and the difficult years of early civilian park administration (1872–86), while the second covers the years of military administration of the park (1886–1918) and the more recent developments under the direction of the National Park Service. Although Haines uses 1886 as a point of division between the two volumes and provides some historic background to most chapters in volume two, his study is really a single volume divided nominally into two parts. Each volume contains its own notes, index, maps, and illustrations; an appendix of prominent explorers and park administrators and a bibliography are located only at the end of the second volume.

Haines bases much of his account of explorations of the Yellowstone region on his earlier book, *Yellowstone National Park: Its Exploration and Establishment* (1974). He credits a number of different people with contributing to the idea of establishing a national park, but he argues that agents of the Northern Pacific Railroad were most responsible for the reality.

This study will be useful to anyone interested in the Yellowstone region, particularly prior to World War I, as well as to anyone who enjoys the stories of mishaps and adventures in Yellowstone's early days. Haines stresses such everyday matters as a stagecoach driver's difficulty in navigating steep park roads and the skiing techniques and equipment of military personnel while on winter patrol.

Despite the wealth of information—indeed, partly because of it—Haines' history is flawed in several respects. The entire manuscript should have undergone rigorous cutting and extensive editing before publication. Haines appears to be an unwitting victim of his many years at the park. He heard so many good stories and interesting anec-

dotes, tracked down so many obscure references, and studied so many park stories and legends that he apparently could not bear to omit many of them.

Haines also overemphasizes some topics and neglects others. For example, he devotes four pages to the story-telling of early trappers, yet he omits the accomplishments of the Civilian Conservation Corps in the 1930s. Although he includes a few references to Horace Albright, he largely ignores the contributions of the civilian park superintendents since 1919 except for brief biographical sketches in the appendix. And he has little treatment of park problems and policies since 1945.

Further, Haines is more successful in volume one in placing the history of park pioneers—fur trappers, miners, and explorers—in the context of the westward movement and national developments than he is in volume two in placing the history of Yellowstone in the context of national park history. For example, he offers relatively little evidence to support his contention that Yellowstone National Park pioneered much that is associated with the national parks.

Haines omits any reference to the only other recent historical survey of the Yellowstone region. Richard A. Bartlett's *Nature's Yellowstone* (1974). Bartlett's book, a history of the Yellowstone from its geological origins to 1872, is the first of a projected two-volume history of the region. Although the prepark histories of Haines and Bartlett are similar in many respects, Haines provides more detail on explorations, while Bartlett includes an extensive section on the natural history of the region. As suggested earlier, however, Haines' second volume is less successful than his first. There is still need for a concise, well-written, interpretive history of the park since 1872.

DOUGLAS H. STRONG
San Diego State University

NATHAN IRVIN HUGGINS. *Black Odyssey: The Afro-American Ordeal in Slavery*. New York: Pantheon Books. 1977. Pp. xvi, 250. \$8.95.

In this well-conceived, wisely informed, and eloquently written essay, Nathan Huggins sketches the drama of those Africans, uprooted by the slave trade, who fused as a people on southern plantations. His theme is the significance underlying the "triumph of the human spirit over adversity." It is as much a philosophical as a historical excursion into the slaves' interior lives to identify their world view. For Huggins, the bondsmen and bondswomen could respond in three ways to the slave trade and slavery—by resistance, adaptation, or opportunism. He theorizes that "after all proper

due is given to the defiant ones, it is to the vast majority who adapted, doing what they could to sustain their humanity despite the cost, that the Afro-American people owe their history" (p. 207).

Although he spent nearly ten years "in a careful study of the surviving records of slaves," Huggins chose "a style often evocative and impressionistic, which departs from the conventional descriptive and analytical exposition of standard histories" (pp. xi-xii). This choice explains the absence of footnotes, but it does not advance the historiography of slavery. An extensive bibliographic essay, perhaps divided by chapters, in which he shared the fruits of his research would have proved more beneficial to scholars. The want of an index, moreover, limits the book's reference value.

Huggins depicts slavery against the backdrop of "tyranny" defined on the American Founding Fathers' terms as the denial of rights to life, liberty, and property without the consent of the governed. By their own standards, the freedom of some Americans was purchased at the expense of other Americans. Consequently, democracy for some meant "tyranny" for others. Huggins is very clear in describing this aspect of American history as a contradiction, not a paradox, that has plagued and continues to confront this country.

While alert to the dangers of generalizing about a continent as diverse as Africa, his description of the African background remains static. He characterizes African society as provincial and Africans as tradition bound with limited vision, reactive more than reflective. In many respects, his Africans are fatalists, resigned to their situation because of forces in nature and society beyond their control. The Africans, in his estimation, lacked a sense of racial consciousness, which in part explains their complicity in the slave trade.

Huggins uses the Africans as a counterpoint to the Afro-American who develops in the New World. Through their resistance to malaria, which isolates them from planters who must seek higher ground during the "sickly season," their fashioning a common language, their adoption of a syncretic Christianity, and their cultural expression, especially music, Afro-Americans emerge as a race-conscious, community-oriented, and resilient people. Huggins identifies the 1840s as the time by which the slaves finally became one people with a single culture. Although fear, deception, and hatred posed dangers to their character, they surmounted these difficulties without resentment. They too were fatalists but in the sense that bondage was their condition and not their destiny. Moreover, they transcended the potential devastation of slavery with their integrity intact. For Huggins, their ethical imperative was duty, "to accept one's duty and to be honest in it" (p. 234).

Their obligation to work for the planter did not, however, imply endorsement of slavery.

This is a provocative book which tells us much that is original about the "world and mind of the slave." It is also the type of study that raises more questions about slavery than it resolves. In this, it makes an excellent teaching device, an introduction bound to stimulate discussion and to bring the subject of slavery vibrantly alive.

ROBERT L. HARRIS, JR.
Cornell University

MERRITT ROE SMITH. *Harpers Ferry Armory and the New Technology: The Challenge of Change*. Ithaca: Cornell University Press. 1977. Pp. 363. \$17.50.

The history of the Harpers Ferry Armory between 1794 and 1861, according to Merritt Roe Smith, provides the basis for re-evaluating some of the persistent myths that have emerged from casual observers of nineteenth-century American technological innovation. Smith's analysis challenges the idea that many American "mechanics" welcomed the change from craft traditions to machine-made products. The agrarian culture that developed around the confluence of the Potomac and Shenandoah Rivers in Virginia spent sixty years contesting technological change. Artisans at Harpers Ferry prized "task-oriented" independence and despised "time-oriented" concern with Yankee productivity, innovative methods, specialized division of labor, standardized products, and uniform accountability.

Though located in a beautiful, bucolic setting, Harpers Ferry emerges from this study as a federally financed Sodom and Gomorrah. Health conditions were bad, transportation and communication systems inadequate, leadership corrupt, cultural and social opportunities nil, violence often present, federal funding inconsistent, worker absenteeism and alcoholism prevalent. Political patronage was necessary for livelihood, the buildings and shops were constructed on a shoddy foundation, and the inhabitants were determined to satisfy their own petty self-interests. Federal investigations continually pointed out that the Armory was run by negligent, if not corrupt, local managers who often falsified records, intimidated workers, took bribes, condoned violence, and sanctioned gambling, drinking, poor workmanship, and cost overruns. No wonder that Smith can claim that "Harpers Ferry's response to industrialization was hesitant and equivocal."

Among the pleasures of this extremely well-written book are the in-depth character studies that Smith provides. Important leaders like James Stubblefield, Colonel Roswell Lee, John H. Hall, and Edward Lucas, Jr. are portrayed with critical

finesse. Even lesser-known mechanics such as Thomas Blanchard, Nahum Patch, Charles Staley, and Thomas Copeland are discussed in a realistic and humane fashion that makes this history exciting and believable.

The chapters on John H. Hall help to clarify a continuing debate over the evolution of the American system of interchangeable parts. Smith provides documentation on the mechanization of the breechloading rifle which Hall developed under government subsidization at a pilot plant in Harpers Ferry. He claims that the introduction of accurate standards, bearing point construction, drop forging, and straight-cutting capacities on huge all-metal machines provided for "the first interchangeable weapons in the United States." The fact that these machines were produced under government contract in a very hostile social and political environment established once more that it was the federal government and not private enterprise which initiated the mechanization of American life. In fact, Smith's chronicle reveals that local craftsmen under the leadership of James Stubblefield and Edward Lucas, Jr. were effective in thwarting productivity. It was not until military management took over Harpers Ferry Armory after the Whig election of 1840 that the master spoils-men were eliminated and the shops reconstructed to meet current industrial standards.

One prominent myth is not critically analyzed in the book. Merritt Roe Smith apparently accepts the inevitability of technological change and the resulting emphasis on efficiency, uniformity, mass production, and time-controlled working conditions. He shows little sympathy for a community based on craft traditions and its struggle for survival. Heroes in this work are "Yankee innovators" who promise accuracy, speed, discipline, and product reliability. John H. Hall's work is considered a major contribution because his labor-saving devices mean that creativity of the workman is no longer a crucial factor. Smith shows little concern for the fact that Hall's devices not only bastardized an art form, but destroyed a whole way of life. The real battle of Harpers Ferry was not John Brown's raid, but the losing battle to develop a viable community under the pressures of our federal government to incorporate this intermediate technological center into a larger industrial network. Though ill-prepared in their social institutions, political experience, and economic leadership, the people of Harpers Ferry did stage a fifty-year battle for the values inherent in the craft traditions. As a result, this study is an important contribution to both urban and technological history.

RAYMOND H. MERRITT
University of Wisconsin—
Milwaukee

HOWARD I. KUSHNER and ANNE HUMMEL SHERRILL. *John Milton Hay: The Union of Poetry and Politics*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1977. Pp. 217. \$9.95.

This slim but thoughtful study of John Hay is the product of collaboration between Howard I. Kushner and Anne Hummel Sherrill, who completed a dissertation on Hay in 1966. Kushner's interest in psychohistory presumably accounts for the occasional, usually cautious speculations about the roots of Hay's behavior. Indeed, the reader may wish for more; it is frustrating rather than helpful, for example, to read that the pattern of one strange illness—and Hay often suffered from poor health—is “suggestive” (p. 69) without being told what it suggests.

To the research incorporated in Sherrill's dissertation have been added new materials, particularly from the Hay Papers at Brown, most useful for the early years, and from secondary works published since 1966. As a result, although State Department materials have perhaps received inadequate attention, this volume rests on a depth of research unusual for one of its length.

Of 124 pages of text, only 37 are devoted to Hay's service as secretary of state. Here, there is little new, although one may quarrel with some of the shadings, including the conclusion—an excessive reaction to claims that Hay was a mindless anglophile—that “as Ambassador to Great Britain and later as Secretary of State, Hay was firm in dealing with the British” (p. 86). The authors devote more attention to his upbringing, particularly the influence of his unambitious but intellectual father and his uncle and sponsor, a driving Illinois lawyer. They provide a good chapter on his service as Lincoln's secretary and the life-long impact of this association upon Hay's outlook and his success in later life. They also describe his middle career, or careers, in diplomacy, business, and journalism and on the fringes of politics.

Kushner and Sherrill are at pains to point out their disagreements with other scholars, sometimes oversimplifying for the sake of effect. They belabor Richard Hofstadter for including Hay in a list of the “downwardly mobile upper-class political reformers of the late nineteenth century” (p. 7), a criticism which, while accurate in the sense that Hay was distinctly upwardly mobile, does not disprove the Hofstadter thesis as a whole. They consider Hay's politico-social outlook at length, demolishing the straw man that in the period following the Civil War he was an authentic radical and, more convincingly, suggesting that the views expressed in his novel, *The Bread-winners* (1883), deserve to be considered carefully and placed in context before he is simply consigned to

the reactionary camp in his later years. Above all they repeatedly assert that, far from being borne upward by mere chance, as Hay himself and some but not all biographers have suggested, he was an ambitious, office-seeking man, “a man who in attempting to control and direct his destiny simultaneously ascribed his success to fortune” (p. 138).

To the text are added twenty-six pages of letters and diary excerpts which, although they lack cohesion, perhaps help to show “a more personal side of John Hay.”

BRADFORD PERKINS
*University of Michigan,
Ann Arbor*

MEREDITH LANG. *Defender of the Faith: The High Court of Mississippi, 1817-1875*. Jackson: University Press of Mississippi. 1977. Pp. viii, 176. \$10.00.

While legal history is no longer dominated by lawyers who recite precedents and call the result history, the decline of legal pedantry should not imply its demise, as Meredith Lang's book vividly illustrates. Lang, a law professor, assumes that opinions of state appellate courts mirror the dominant moral and intellectual climate of the society which they serve. Thus, in antebellum Mississippi the high court reflected the state's transformation between 1817 and 1860 from the open to the closed society. This transformation, which Lang interprets as the triumph of parochialism over nationalism, produced a court “devoted to the three articles of faith on which Mississippi society rested: slavery, state sovereignty and the compact theory of government” (p. viii).

Individual chapters trace the court's decisions in these areas and examine as well its opinions on criminal law, the impairment of contract clause in the state constitution, and Reconstruction. Not surprisingly, Lang finds that the court was most sensitive to societal values in cases concerning slavery, especially manumission. Here “legal rationalizations were thin veils to cover fixed prejudices, and the emotional context of the opinions deprives them of any intellectual respect” (p. 158). But in criminal law, where Lang sees no fundamental threat to the institution of slavery, the court adopted a humane and enlightened policy, even toward the slave himself so long as the criminal behavior was not rebellious.

Historians will recognize nothing new in these conclusions. Indeed, some legal scholars will find evidence here to support their own research on antebellum law. But the author fails to go beyond court decisions in order to examine the circumstances which produced them or the impact of those decisions on society. An assertion that appel-

late opinions reflect the priorities of society is a statement that must be tested. Lang cannot do this, however, because she has consulted only a minute fraction of the source material available to her. Only 7 of 424 footnotes refer to sources other than court cases or sessions acts. The bibliography, moreover, lists just one newspaper—the *New York Times* for October 26, 1866—and two articles, none of which are cited in the text. Nor has the author used the work of other legal scholars who have studied similar materials. The chapter on Reconstruction, for example, contains no reference to any secondary source, much less to valuable legal histories by Hyman, Paludan, Kutler, and others.

Inadequate research is only one weakness in this much-flawed book. Too frequently, cases are merely compiled and summarized, and when analysis is attempted, it is often uninformed or simply incorrect. Conclusions are banal and sometimes contradictory. Lang's court evidently functioned without judges, for she tells us nothing about them. "What if" questions engage the author's attention too much; these questions are fun to ask, but they rarely produce good history. Finally, Lang's style is, by turns, either trite, awkward, or pretentious. In sum, this book demonstrates once again that lawyers are not necessarily legal historians.

DAVID J. BODENHAMER
University of Southern Mississippi

HAMBLETON TAPP and JAMES C. KLOTTER. *Kentucky: Decades of Discord, 1865–1900*. Frankfort: Kentucky Historical Society. 1977. Pp. xvi, 552. \$17.50.

With this volume, Hambleton Tapp and James Klotter have made an important contribution to the writing of Kentucky history. As has been the case with so many Southern states, scholars have carefully worked the pre-Civil War history of Kentucky but have largely ignored the years since Appomattox. The third volume of a projected four-volume study of the state being prepared by the Kentucky Historical Society, *Decades of Discord* is a major step toward repairing that neglect. It contains interesting material on social and cultural activities as well as politics and is very handsomely produced, with well-chosen illustrations and useful appendixes. A concluding bibliographical essay provides a good summary of the available literature that should be especially helpful to those contemplating research in this area.

Most of the weaknesses of this study can be traced to the fact that the authors had so few monographs to draw on. For example, while they deal with blacks in Kentucky, they say very little

that is helpful about the creation of the segregation system in the state. They discuss the beginning of mining in eastern Kentucky without offering any significant analysis of the process. Their chapters on politics are informative, but they tend to become repetitious accounts of elections and legislative sessions that do not explore emerging patterns. These areas have not received much attention from Kentucky historians, but the authors have, however, overlooked publications that would help to place Kentucky in a wider regional and national context. Their bibliography does not indicate that they consulted comparable studies of other states, nor do they take much note of the numerous studies dealing with national questions that have been produced in the last several years. This is especially true with regard to Populism. As a result, they have missed an opportunity to set their work against a broader and more useful backdrop.

None of this detracts from the importance of what Tapp and Klotter have done. Despite occasional gaps, they present a vivid portrait of a violence-torn society struggling to stabilize itself in the aftermath of civil war. Among the first to attempt a synthesis of this convoluted and difficult period in Kentucky history, they have done admirably in spite of major obstacles. Their work organizes the existing material while suggesting an agenda of research for future scholars.

DAVID D. LEE
Western Kentucky University

JOHN H. KEISER. *Building for the Centuries: Illinois, 1865 to 1898*. (The Sesquicentennial History of Illinois, number 4.) Urbana: University of Illinois Press, for Illinois Sesquicentennial Commission and Illinois State Historical Society. 1977. Pp. xvi, 386. \$12.50.

Building for the Centuries is concerned with how and, to a lesser extent, why Illinois changed from a rural to an industrial state, with accompanying changes in values and social and political developments which lagged behind technological transformation.

The topic of this monograph as outlined above, namely industrial and economic development with resulting cultural lag, is probably one of the most important topics dealt with in historical works. The results of such studies are usually somewhat depressing because cultural lag is one of the major banes of civilization. But I know of no better way to ameliorate cultural lag than to have historians describe it in a historical context in the hope that people may learn from history. Finley P. Dunne's Mr. Dooley rather aptly and pessimistically

pointed up the problem when he said, "A man that'd expect to thrain lobsters to fly in a year is called a loonytic; but a man that thinks men can be tur-rned into angels be an illiction is called a rayformer an' remains at large" (p. 281).

In my opinion, this study is well done. But I must admit to a bias, since *Building for the Centuries* is written in much the same format as my own *History of the Middle West*. First, John H. Keiser has organized his book topically. He has chapters on the structure of government, politics, agriculture, transportation, industry, labor, population, education, medicine, culture, and Chicago. This last topic does not quite fit in with the rest; however, one can understand why the author devotes a separate chapter to Chicago, since it is literally the tail that wags the Illinois dog.

Second, also in accordance with my own proclivities, *Building for the Centuries* is loaded with statistics, most of them excellent. A few examples will suffice: "Money spent on advertising increased by more than 1,000 per cent between 1867 and 1900, indicating the national 'brand name' drive" (p. 3). "Between 1870 and 1900, the population of Illinois nearly doubled, while the number of workers in manufacturing quadrupled and the capital invested in industry multiplied over eight times" (p. 180). By 1900 "Illinois had become the nation's third-ranked industrial state, and its industrial products doubled in value those of its farms" (p. 322). Occasionally, however, the significance of the statistics becomes questionable, as in the statement that "there were 15,000 residents in Springfield in 1865. Forty of them owned Singer sewing machines" (p. 36).

Third, this work contains some fine sketches of colorful people and events in Illinois history. Examples include Senators Lyman Trumbull and Shelby Cullom, Governors John Tanner, John P. Altgeld, and Richard Oglesby, Mayor Carter H. Harrison, political bosses John Coughlin and Michael Kenna, social worker Jane Addams, meat packer Philip D. Armour, architect Frank Lloyd Wright, attorney Clarence Darrow, editorialist Finley P. Dunne, businessman Marshall Field, writer Carl Sandburg, and radical August Spies. Among the events emphasized are the development of barbed wire, the Illinois Constitution of 1870, the Chicago fire of 1871, the Haymarket riot, the Pullman strike of 1894, and the World's Columbian Exposition.

The documentation and bibliography of *Building for the Centuries* is excellent. The research is based heavily on original sources, especially contemporary newspapers. In addition there are seventeen pages of appendixes and twenty-two pages of primary and secondary sources.

I would strongly recommend this work for in-

clusion in public, undergraduate, and graduate libraries, especially those containing state and local history collections.

KENNETH R. WALKER
Arkansas Tech University

LYLE W. DORSETT. *The Queen City: A History of Denver*. (Western Urban History Series, number 1.) Boulder, Colo.: Pruett Publishing Company. 1977. Pp. 320. \$12.50.

Lyle Dorsett's account of Denver is informative, interesting, well researched, and lively in its descriptions of people and events. It takes the city's history from hard-scrabble beginnings in the mid-nineteenth century through the first burst of growth in the several decades after 1870, the problems of adjustment after 1900, and the renewed pressures for expansion after World War II. The book is especially good on business developments and financial dealings and devotes ample attention to women, minorities, public assistance, and cultural life. It is generally weak on urban transportation, social structure, the spatial dynamics of urban growth, and Denver's regional role.

Situated on the eastern slope of the Rocky Mountains, Denver was far removed from the nation's industrial core. But the city lay in the heart of a mineral-rich region, and thousands of people came seeking the city's healthful climate or an often elusive vision of success. To a greater degree than with most cities, Denver's development was influenced by outside capital and an influx of federal offices and installations. In the half century after 1870—when early leaders fashioned the crucial rail connections that secured a promising urban future—Denver's population grew from less than five thousand to more than two hundred fifty thousand and doubled in the following fifty years. Growth might have been even greater in the early twentieth century but for the desire of the established elite to discourage the "wrong kind" of people and industries and maintain its comfortable dominance.

This urban "biography" puts the emphasis on personal biography. Divided into five chronological parts—each containing a chapter on local leaders and one on "quality of life"—the book provides a more detailed appraisal of the elite than we usually get nowadays, and this feature will doubtless attract the interest of local history buffs as well as scholars. But the arrangement becomes almost schizophrenic, alternating between fulsome praise for the determination and skill of the bankers and businessmen and bitterness at their profiteering and neglect of the city's unfortunate. Dorsett reminds us of the importance of individual personal-

ity and talent, though the emphasis seems occasionally overdrawn. His characterizations of two twentieth-century mayors, Robert W. Speer and Benjamin F. Stapleton, border on panegyric. And I think he also tends to overemphasize beautification and recreation projects in assessing the city's overall "quality of life." Dorsett understandably condemns the "contagious disease" of growth at any cost that pervaded Denver after 1945; but his apparent sympathy for the "controlled growth" practiced by the city's established leaders so clearly in their own narrow interest is a bit troubling.

This book is not a detailed structural analysis of urban space or society, but certain terms like "power elite" are tossed about a bit loosely even for a general account. And the lack of any graphic materials and maps is a grave disadvantage to readers interested in the distribution of neighborhoods and economic functions, the fluctuations of population, the impact of urban transportation and utilities, and the complex social fabric.

BLAINE A. BROWNELL
*University of Alabama,
Birmingham*

JAMES E. HANSEN II. *Democracy's College in the Centennial State: A History of Colorado State University*. Fort Collins: Colorado State University. 1977. Pp. vii, 494. \$19.95.

Anyone who has ever studied the history of the land-grant-college movement in the United States will find much that is familiar in the first six chapters of James E. Hansen's *Democracy's College in the Centennial State*. From its foundation at Fort Collins in 1879, Colorado Agricultural College experienced most of the problems so common to the early history of similar institutions in other states. Besides internal bickering, inadequate funding, intermittent parochial interference, and pressure from special interest groups like Grangers and stockmen, the fledgling school was viewed with suspicion and some hostility by other state institutions (University of Colorado and the Colorado School of Mines). Its most perplexing problems, however, grew out of efforts to define its purpose. Hansen does an excellent job of presenting what he calls (using terminology borrowed from Earle D. Ross) the "broad" and "narrow" gauge theories of industrial education. In Colorado's case, proponents of the former position finally prevailed but only after a bruising public battle in 1908.

Unlike many authors of institutional histories, Hansen is careful to tell the story of Colorado State University in the context of larger national and regional events. His account of developments lead-

ing to the passage of the Morrill Act in 1862 and his description of Colorado geography and territorial history are particularly skillful. So are his background discussions for both world wars and for campus unrest during the 1960s. For those readers who are not alumni, there is probably too much attention devoted to individual faculty members, administrative politics, and football schedules. But no doubt the university had the alumni in mind when it published this volume. Copiously illustrated and printed on glossy paper, the book more nearly resembles a college yearbook than a monograph. But that does not detract from its scholarly value. Carefully researched, objective to a fault, and clearly written, the book certainly merits a place in the library of any college or university with a special collection on the history of education.

GERMAINE M. REED
Georgia Institute of Technology

JAMES C. CAREY. *Kansas State University: The Quest for Identity*. Lawrence: The Regents Press of Kansas. 1977. Pp. xii, 333. \$10.95.

James C. Carey informs us that this institutional history was written for "K.S.U. alumni, students, faculty, and friends" (p. xi). It also serves a broader audience as a discerning case study for the many "A. and M. schools" resulting from the Morrill Act of 1862. Kansas had been the first to designate its land-grant institution by transforming Bluemont Central College from "that little Methodist school" into Kansas State Agricultural College in 1863.

In efforts to capture the personality of the succeeding 115 years in a single volume, the author has concentrated on an interpretive history that emphasizes the school's quest for identity. Its laborers had to "create a land-grant college without benefit of a model anywhere in the world" (p. 40). But slowly, sometimes painfully, an educational pattern took shape, consisting of resident instruction, research, and extension services. In the process, American expansion of educational opportunities beyond traditional European caste and elitist constituencies was further widened. But not until the presidency of Milton S. Eisenhower (1943-50) following World War II did the college emerge from the more restrictive aspects of technical school philosophy.

Thereafter, the Kansas State campus was increasingly a part of the wider academic milieu. It shared the quiet fifties and those enigmatic sixties that brought the golden bonuses of enrollment and curricular expansion and the difficulties and challenges of student activism. At present, the author

concludes, the school's identity is that of a multi-purpose university, helping the students in "making a life as well as making a living" (p. 282).

Carey's faithful adherence to his announced goal of writing an interpretive history has provided a meaningful study. To call attention to omitted aspects is therefore more an informative report to prospective readers than a criticism. The various academic divisions, for example, are not adequately explored, not even those of agriculture and home economics in a prominent land-grant institution. Newer colleges such as business and education receive only three or four lines each. Involvement of an administrative framework is not traced. Students never quite come to life, either in classroom or extracurricular activities.

The merits of this book exemplify the kind of contributions made by Clio's disciples since assuming responsibility for writing college histories during the past generation. It is now time to go beyond the individual school and explore the larger, diverse world of American higher education.

CEDRIC CUMMINS
University of South Dakota

THOMAS D. CLARK. *Indiana University: Midwestern Pioneer*. Volume 3, *Years of Fulfillment*. Bloomington: Indiana University Press. 1977. Pp. xxii, 678. \$19.00.

Like the preceding two volumes, *Years of Fulfillment* achieves high standards in thoroughness and amplitude of research, conceptualization, and exposition. The reader is seldom at sea in the mass of detail about the complicated problems of financial support, the never entirely adequate expansion of the plant, administrative organization and reorganization, instruction, programs, research, student life, including athletics, and the development of the professional schools and the four-year campuses. Thomas D. Clark solved the problem of keeping our heads above water by never allowing us to forget the central question: how did an institution whose ablest minds in the 1930s regarded it as mediocre in relation to state universities in neighboring states and to Indiana's resources become a truly great university in the short span of the time when it also became a very large one?

Due weight is given to the contributions of the federal government in the New Deal and during World War II and its aftermath. Indiana did a better job of taking advantage of federal largess than some of its sister institutions. It made the most of new opportunities for broadening its mission of public service from state and region to the nation and the world. Indiana's achievements also

owed a great deal to the unusual dedication of many men and women who spared no criticism in brutally candid self-evaluation of the institution and no effort to create a great university.

No one, of course, can say what would have been accomplished without the leadership, ability, work, and dedication of President (later Chancellor) Herman B. Wells. His contributions cannot easily be summarized. They included his zest and good judgment in rebuilding the faculty by establishing an adequate retirement system and by recruiting distinguished scholars and scientists. He managed against odds to build an ever-expanding, modern campus. Common sense, sound judgment, and genuine interest marked his relations with students, faculty, trustees, and alumni. He was astute and wise in his relations with the legislature and with pressure groups determined to restrict academic freedom. He did much to make Indiana proud of its university. Without uprooting the best in its traditions, he did more than any other single person in transforming a parochial campus into a distinguished, cosmopolitan one.

Any historian will find especially interesting the superb chapters on academic freedom, the Kinsey studies, the development of the great school of music, and the time of troubles in the 1960s.

It is hard to think of any way in which this book could have been better, unless it might be to wish that Clark had greatly expanded his brief paragraph on the achievements and careers of Indiana's graduates. Even so, they like all other Hoosiers may take pride not only in their university but in the highly readable three-volume story of its development. *Indiana University: Midwestern Pioneer* is unexcelled in the historiography of American higher education.

MERLE CURTI
*University of Wisconsin,
Madison*

ARTHUR J. MAY. *A History of the University of Rochester, 1850-1962*. Edited and abridged by LAWRENCE ELIOT KLEIN. Rochester, N.Y.: University of Rochester. 1977. Pp. x, 359. \$20.00.

This book is a good, solid account of the University of Rochester from its founding in 1850 to 1962. Despite the manuscript's somewhat checkered history (the author died before he finished the first draft so several others were involved in completing it), the work is, in general, very well executed.

Arthur May covers with care and thoroughness each segment of the university's growth. He follows the traditional path of organizing his material around each of the institutions's presidents. The rise of the university's various schools and depart-

ments is carefully detailed. May attempts in a worthy, if not entirely successful, effort to show how the growth of the city of Rochester influenced the development of the university in its midst. He also tries to keep the reader aware of developments in higher education as he recounts the history of the university, but his comparisons are usually in very broad terms. For example, May includes an excellent section on the incredibly boorish behavior of Rochester's male undergraduates toward the women who entered the college when it became coeducational. He refers to the beginnings of coeducation elsewhere, but he appears to have made no attempt to discover if other male students behaved like those at Rochester.

From the point of view of the student of higher education, the book has some shortcomings. It has no footnotes, and the bibliographical essay is too brief to be of much use. There are, in addition, times when May's sources appear to be weak. He seems to have leaned excessively on school newspapers and not to have used outside material enough. The undergraduate newspaper is surely not the best source for information about early cataloging practices in college libraries.

The general reader, as well, will have his criticisms. Detail is sometimes uselessly piled up (we do not really need to know how many miles corridors would stretch if laid end to end), the writing lacks verve, and it is sometimes graceless as well. But these shortcomings are, on the whole, outweighed by the fact that the volume contains a wealth of information which is presented in an honest and straightforward fashion. There are sections that are very well done indeed. No doubt it would have been a better book had May lived, but nonetheless the University of Rochester can be well pleased with this history.

BROOKS MATHER KELLEY
Yale University

CHARLOTTE WILLIAMS CONABLE. *Women at Cornell: The Myth of Equal Education*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 211. Cloth \$12.50, paper \$3.95.

Although this survey of the educational fate of women at Cornell since the institution's founding in 1868 spans a full century, historians will be most interested in the discussion of the ironies that affected Cornell women students in the first two decades of the period. Conable shows how closely interwoven were efforts to expand education and to emancipate women in upper New York state. She argues very persuasively that the founders—Ezra Cornell, Andrew D. White, and Henry W.

Sage—from the beginning intended Cornell to be coeducational, thus refuting the thesis that links admission of women to the original charter's accidental reference to "persons" instead of "men." It is ironic, given the "protected" status that women would eventually be assigned, that the founders believed that women needed higher education to increase their self-sufficiency in situations where a husband's death or desertion forced them to support their families. Despite the founders' intentions and the charter's commitments, only intense pressure from President White and Henry Sage's offer of a magnificent dormitory for women students moved the trustees to approve coeducation in 1872.

Conable's thesis—provocative if not entirely convincing—is that Sage College, the very building that made women's admission possible, developed into a kind of jail for them. College educators had long debated whether women students should, like men, choose their own living arrangements or whether they should be segregated in supervised dormitories. This volume is especially good in describing how the economics of empty beds decided the issue at Cornell. At the start, Cornell's supply of living facilities for women outstripped demand. Originally Sage College housed only those women who chose to live there, and they lived there without supervision. Women could also choose to live at home or in local boarding houses. In 1884, however, the trustees, responding to Cornell's precarious fiscal condition, to the chronic underutilization of space in Sage College, and to late-Victorian qualms about the unsupervised female, required all female students to live at Sage College, imposed rules of behavior on all coeds, and appointed a directress to enforce them. This policy (which remained in effect into the 1960s) was the first to require differential treatment of female and male students and presaged a wholesale discriminatory attitude that would touch every aspect of women's experience at Cornell. The policy set an absolute limit on female enrolment, and when the female dormitories did fill to capacity, Cornell had to deny admission to more qualified women candidates than men candidates. By undermining women's sense of independence and self-responsibility, the policy diminished their ability to benefit from higher education and tacitly supported attitudes, well represented among the male student body, that regarded women as dependent and subservient. Here the book is least satisfactory, jumping rather hastily from dormitory policy to fraternity exclusion of men who dated Cornell coeds to discrimination in career preparation. The author fails to compare Cornell to other Ivy League schools which moved in the late nineteenth century to house and supervise all students on campus. Nor does she supply sufficient information

about the social context of the university's attitudes toward women, contending, indeed, that attitudes toward women hardly altered between 1884 and 1960.

The volume describes how Cornell's discriminatory policy against women began more clearly than it explains how it was sustained so effectively for so long. The author says little about why those patterns did begin to change in the 1960s. The last portion of the book is somewhat thin—a mixture of portraits of successful Cornell alumnae, snippets of social history about hazing and dating procedures, and some results from a questionnaire the author administered to alumnae. An appendix giving representative answers to questions such as "What have proven to be the most relevant aspects of your Cornell education?" and "How did the coeducational experience affect your life?" provides a valuable resource for historians of women in higher education. This volume is similarly a most valuable resource—as good an account of the changing experience of coeducation as is available today.

ROBERT L. CHURCH
Northwestern University

BRUCE KUKLICK. *The Rise of American Philosophy: Cambridge, Massachusetts, 1860–1930*. New Haven: Yale University Press. 1977. Pp. xxvii, 674. \$17.95.

This imposing volume provides a meticulous, and at times microscopically detailed, account of the development of modern philosophy at Harvard. Bruce Kuklick is interested in institutions as well as ideas. From both the institutional and the intellectual points of view, Harvard's importance justifies the close attention he gives it, for as Bertrand Russell said, Harvard's department of philosophy in the age of James and Royce was "the best in the world" (p. 242). Its students were the leading figures in the first generation of professional philosophers, and the transformation of both intellectual and institutional style that occurred at Harvard around the turn of the century epitomizes the course of academic professionalization everywhere.

The author struggles to keep the institutional and the intellectual parts of his account running side by side, and if he never quite succeeds in that difficult task, at least it can be said that each aspect of his story profits from being examined in conjunction with the other. On the intellectual side, his major theme is the development of a tradition of discourse that originated in antebellum Unitarian philosophy and reached fruition in pragmatism. The tradition drew energy from two main concerns: first, a desire to rescue philosophy from the skeptical impasse to which it had been brought by David Hume, and second, the effort to

find a sound basis for religion in Darwin's universe. In both tasks Harvard philosophers found Kant the indispensable point of departure. Kuklick takes pains to demonstrate the centrality of Royce and the strength of idealism generally at Harvard, especially in the development of pragmatism. It is a special merit of the book that he avoids collapsing a thinker's work into a single position, instead deftly tracing each man's development over a path of many twists and reverses.

On the institutional side, Kuklick's principal theme is professionalization, about which he acknowledges a deep and justifiable ambivalence. He admires such preprofessional thinkers as Francis Bowen, Chauncey Wright, and Francis Abbot, all of whom addressed the largest questions of their day in a manner that a later generation of scholars would consider undisciplined and even reckless. Kuklick attributes the "Golden Age" of Harvard to the individual genius of James and Royce and laments the tame pedantry and careerism that prevailed after their death. Yet he cannot help admiring also the concentration and self-restraint of a later professional such as C. I. Lewis, who confined himself to technical questions, fearfully aware that a scholar outside his specialty "can be as much of a Goddam fool as anyone else" (p. 561). Like others who have studied the professionalization of scholarship, Kuklick is impressed by the greater rigor of argumentation that it brought, and yet repelled by the narrow scholasticism that it also fostered.

In his own choice of audience, Kuklick is unambivalently professional. He concedes that his chapters dealing "with the complexity of ideas reflect one crucial dilemma of professional philosophy—its inability to communicate with a non-professional readership" (p. xxvi). Yet one wonders how hard the author tried to communicate with nonspecialists. His first hundred pages, devoted to the controversy over evolution, make splendid reading, and throughout one finds passages of great clarity; but there are long stretches that do not repay the time and patience it takes a nonspecialist to decipher them. Insurmountable technical difficulties may explain the unengaging quality of a discussion of a philosopher like Royce, but the reader who finds himself reaching for an original text by William James in order to clarify what is supposed to be a summary may prefer to blame a prose style that is careless of the needs of nonspecialists. It will be an ironic and I think unnecessary triumph of the most regrettable aspects of specialization if valuable studies of professionalization like this one are allowed to become intellectually inaccessible to all but those who specialize in the field under investigation.

THOMAS L. HASKELL
Rice University

MARION L. BELL. *Crusade in the City: Revivalism in Nineteenth-Century Philadelphia*. Lewisburg, Penn.: Bucknell University Press. 1977. Pp. 299. \$15.00.

Marion Bell has assembled an impressive number of sources relating to the religious life of Philadelphia during the nineteenth century in order to study revivalism "within the context of industrialization and urbanization" (p. 14). Such a study is surely needed. Most of our knowledge about revivalism has been derived from studies of rural areas, yet the incidence of revivals in cities is striking. Unfortunately, *Crusade in the City* does not fill this need.

The organization of the book does not lend itself to the analysis promised in the introduction. Rather than a central argument which is systematically sustained through sequential chapters, we find loosely related essays. In the first chapter we find a collection of well-known material about Charles Grandison Finney and a few interesting bits of information about his correspondence with churchmen in Philadelphia prior to his brief visit to that city in 1827-28. The second chapter is a discussion of the social fabric of the city and the state of religious life in the 1820s. Those readers familiar with Sam Bass Warner's analysis will find nothing new in the discussion of the social fabric. Although the author presents interesting information, hitherto generally unavailable, about religion in Philadelphia, her most useful insights are paraphrased from more general works by such scholars as Winthrop Hudson.

Interesting and original material is again introduced in the third chapter, a discussion of the Finney and post-Finney revivals in early nineteenth-century Philadelphia. The one interpretive thrust in the chapter is the suggestion that the uneducated lower classes were most affected by the revivals. This suggestion is based on fragmentary and impressionistic data. Furthermore, it is contradicted by the major theme of the fourth chapter that "it was the young people who were dominant in the turbulent revivals . . ." (p. 78). Again, fragmentary and impressionistic data are presented, coupled with a brief discussion of the theoretical framework for comprehending youth provided by Erik Erikson.

Chapters six through ten contain a potpourri of information on religious life in Philadelphia: voluntary associations, sectarian differences concerning revivalism, further information on post-Finney revivalism, and two chapters that seem curiously out of place. The discussion of black revivalism is informed primarily by the theoretical assumptions of Herskovits and DuBois and is unrelated to the discussion of revivalism in the rest of the book. The chapter on the "Outsiders," those untouched by the revival movement, is similarly

unrelated to the rest of the book. The eleventh chapter, similar to the first and third, consists of a rehashing of what is generally known about the career of Dwight L. Moody and interesting information on his brief visit to Philadelphia. This chapter, the most useful in the book, contains fascinating material on the role of such local business worthies as John Wanamaker in the Moody revival. The book concludes with a brief statement of the social control function of revivalism, which has been discussed often during the last three decades.

Crusade in the City depends too heavily on a "great man" approach, giving too much attention to Finney and Moody. Answers to the questions concerning the relationship between revivalism, urbanization, and industrialization are not given; nor are they suggested. Indeed, the questions are never formulated. In spite of this reviewer's misgivings, the information contained in the book is bountiful enough to interest urban and religious historians. We must still wait, however, for the book which will deliver what Bell promises in her introduction.

GREGORY H. SINGLETON

Northeastern Illinois University

R. LAURENCE MOORE. *In Search of White Crows: Spiritualism, Parapsychology, and American Culture*. New York: Oxford University Press. 1977. Pp. xvii, 310. \$12.50.

Until the last decade historians, with a few notable exceptions, have left nineteenth-century spiritualism and its successor, psychical research, to polemicists and anecdotalists. More than merely a sign of increased scholarly interest, R. Laurence Moore's *In Search of White Crows* goes far toward redeeming this neglect. His title is prompted by William James' remark on the reluctance with which science, believing all crows to be black, assimilates a single controverting "white crow." Moore traces the unsuccessful attempt of both movements, from the Fox sisters on, to find evidentiary white crows which the cultural "mainstream" would accept as significant fact.

Subordinating actors and events to the intellectual and social patterns he has heroically wrested from what knowledgeable readers will agree is "an almost infinite number of tracts," Moore attributes spiritualism's nineteenth-century popularity to its wishful scientism (the "spiritual telegraph" of table rappings was supremely empirical). He thoroughly explores both the response of orthodox churchmen who feared the movement's democratizing and rationalizing influence and the attraction of other-worldly encouragement for social reformers who had experienced frustration in this world. Moore closes his topical

analysis of spiritualism with a vivid sketch of the "female medium's" professional career—an occupation defined by cultural notions of passive and fragile femininity and often accompanied by fraud and self-deception, yet one which offered travel, attention, a sense of power and purpose, and for some mediums the opportunity while in trance to express feelings and assume roles usually denied to women.

The second half of Moore's book is a generational narrative of psychical research from its auspicious beginnings under William James' leadership in the 1880s, through the heavy handed direction of James H. Hyslop, the "Margery" embarrassment of the 1920s, and the reductive pursuit of "psi phenomena" by J. B. Rhine (who renamed the movement "parapsychology"), to the "occult revival" of the 1960s. The tale is a melancholy one. Eighty years ago psychical research commanded no little respect for its exploration of the anomalies of consciousness. But despite the recent recrudescence of popular curiosity about the paranormal, the latter-day movement, as described by Moore, is rudderless, conflicted, and aware that it has "failed to revolutionize science with white crows." Moore suggests that spiritualism and psychical research alike have too easily discounted their affinities with occult tradition—affinities which made scientific acceptance unlikely from the beginning.

Neither spiritualists nor psychical researchers may take kindly to Moore's sympathetic explanation of their "cultural deviance." Scholars will question this or that emphasis. I think, for instance, that Moore gives relatively too much weight to the concern for science among those who flocked to nineteenth-century séances and that he touches too lightly on fraud and credulity (admittedly an issue which has too often preoccupied other writers). The contrast, moreover, between his leveling approach to spiritualism and his attention to individual psychical researchers may strike some readers as dissonant. But although Frank Podmore's more colorful if less systematic *Modern Spiritualism* (1902) remains the necessary introduction, *In Search of White Crows* is the best full-length assessment of American spiritualism since Podmore's, which it surpasses in interpretive breadth. And it stands alone as the only worthwhile historical account of American psychical research.

HOWARD KERR
University of Illinois,
Chicago Circle

JOSEPH E. KING. *A Mine to Make a Mine: Financing the Colorado Mining Industry, 1859-1902*. College Sta-

tion: Texas A&M University Press. 1977. Pp. xv, 209. \$13.75.

In 1859 John H. Gregory discovered an outcrop of a gold vein in Colorado and thereby inaugurated the area's mining industry. Prospectors like Gregory, however, do not figure prominently in this study. Joseph E. King's book is not Gold Rush romance or adventure but a solid business history of the "mobilization, migration, and impact of American, principally eastern, capital on a major state in the mining West" (p. xv).

Following a short-lived boom after 1859 the Colorado mining industry fell on troubled times until sophisticated technology and absentee capital came to the rescue. King examines how professional promoters, the aspirations and associations of individual eastern investors, and mining booms in Leadville and other camps brought outside money to Colorado. Such investment ran notable risks. Indeed, easterners were as much victims of exploitive promotional schemes and mine mismanagement as exploiters of the Colorado mining industry. The hazards of investment were numerous and included overcapitalization, stock assessments, distorted production reports, company reorganizations, communication problems, and litigation. The author concludes that "whatever perils threatened a mining enterprise, from financial finagling in the East to legal assaults in Colorado, the investor ultimately ran the risks and paid the bills" (p. 149).

What emerges from this first comprehensive and scholarly study of eastern capital in Colorado is a generally sympathetic interpretation of absentee mine owners who, until the decline of production at Cripple Creek in 1902, apparently gave more money to Colorado in expenditures for supplies, services, and labor than they took out of the state in profits. Eastern capitalists opened new mines and extended old ones, supported the trial and error experimentation necessary to find a solution to the problem of processing Colorado's "refractory" ores, facilitated local investment in the state's mining industry, encouraged the exploration of mineral resources and business opportunities elsewhere in the West, and fostered the economic integration of the country.

Although the book is ably illustrated with pictures of absentee capitalists such as Norvin Green, Henry B. Hyde, and Cyrus McCormick, a table listing these men and others by their primary places of investment would have been appreciated. In addition, the author could have extended the chapter on the hazards of mining investment to include discussion of labor problems and uncertain or costly transportation to market. An apparent contradiction also arises when King indicts

George D. Roberts for his unscrupulous promotion of Leadville mines but defends professional promoters as indispensable to the survival of the Colorado mining industry. Still, these are minor criticisms which should not detract from the overall value of the study.

Based on extensive research in such primary sources as the manuscript papers of selected investors and written in an engaging, lively style that makes good use of graphic quotations, the book is a significant contribution to mining history and an impressive affirmation of the proposition that the West can best be understood not by itself but in relation to the rest of the nation.

RICHARD H. PETERSON
San Diego State University

RICHARD H. PETERSON. *The Bonanza Kings: The Social Origins and Business Behavior of Western Mining Entrepreneurs, 1870-1900*. Lincoln: University of Nebraska Press. 1977. Pp. xvi, 191. \$9.95.

Richard H. Peterson chose the fifty men who are included in this study largely because of their financial success. He does not claim that they are necessarily the most important of the "bonanza kings," but only that they are not unrepresentative of the group. The period covered, the last three decades of the nineteenth century, was the time during which the western mining frontier ceased to be the domain of the wandering prospector and became that of Peterson's entrepreneurs, as mining developed into "a technologically sophisticated industry" with "an increasingly stable population base and labor force" (p. xiii).

Peterson's aims are to account for the success of the western magnates, to discover how closely they fitted the popular stereotype of mining frontiersmen (individualistic, self-sufficient, and violent), and to determine how nearly the westerners conformed in their social origins to "national, mostly eastern, business elites" (p. 3). He begins with the last comparison, using for his purposes two articles on the social origins of American businessmen by Frances W. Gregory and Irene D. Neu, and by William Miller. The groups in the Gregory and Neu and the Miller studies (both of which appeared in *Men in Business*, edited by Miller [1952]) are not quite comparable with Peterson's bonanza kings, for the earlier studies include *all* the holders of the top offices in the largest units of designated businesses, whereas Peterson's group is both smaller and less systematically chosen. Peterson concludes that, unlike the eastern businessmen, who began their careers from the plateau of the middle class with all its advantages, the western

mining magnates exemplify the traditional view of big businessmen of the Gilded Age as working their way up from poverty. It would appear, then, that the frontier provided opportunity for greater social mobility than did the older, longer-settled areas of the country. Once the westerners had achieved success they differed little if at all from their eastern counterparts. They lived in Victorian mansions, belonged to exclusive clubs, traveled in private railroad cars, and spent their vacations at Newport.

Peterson makes clear that cooperation among mine owners, understanding between owners and workers, and a preference for peaceful conditions in the mining country characterized the behavior of the majority of the bonanza kings. At the same time he admits that contentious competition, harsh labor policies, and use of violence were the hallmarks of a highly visible minority.

One factor in the moguls' success was technological education, gained in formal institutions or on the job. Early arrival in the successively opened mining areas was another factor, although Peterson's subjects, like many other wise promoters, were reluctant to bear the initial costs of technological innovation. But even well-tried methods required capital investment, and access to moneyed partners greatly enhanced an entrepreneur's chances of success. Only slightly less important, perhaps, was the ability to identify promising mining property, to spread one's risks through geographic diversification, and to recognize the proper time for selling out.

There are many books on the western mining frontier, but this is the first that is concerned with the social origins of the mining magnates as a group and the specific factors in their success. As such it is a commendable addition to our understanding of the evolution of Western mining from "pick and shovel" prospecting to big business.

IRENE D. NEU
Indiana University,
Bloomington

ROGER L. GRINDLE. *Tombstones and Paving Blocks: The History of the Maine Granite Industry*. Rockland, Me.: Courier of Maine Books. 1977. Pp. xi, 277. \$11.95.

Before steel beams and reinforced concrete revolutionized building construction, millions of tons of granite were used in public and private buildings, and for many other purposes—from tombstones to paving blocks. Roger L. Grindle's book, which deals principally with the period 1877-1914, when Maine was one of the leading granite-producing states, suggests that the industry has many facets that would make for an intriguing story. Maine

fishermen and farmers, joined by immigrants from the British Isles and Italy, quarried rock, not only at inland sites, but on small, bleak, windy islands. Living in company-owned boardinghouses and cottages, and shopping in company stores, these isolated laborers were often at the mercy of unscrupulous employers, who, on occasion, even marched them to the polls. Because of ethnic conflict and seasonal employment, unionization was difficult, but some labor organizations were moderately successful. Despite the unions, however, the quarryman's world was always full of hardship and danger. Each year men were crushed by massive stones; others fell from slippery perches; a few were torn to bits by dynamite charges. Granite cutters and dressers lived less harrowing lives until time saving inventions, such as pneumatic carving tools and sandblasting machines, produced the fine dust that caused chronic lung diseases and reduced life expectancy.

Unfortunately, Grindle has organized his study in a manner that is very difficult to follow. By describing the conditions of labor at one quarry after another, first for the 1880s, and then for the 1890s, and again for the early twentieth century, his presentation often amounts to a catalog of unrelated events. His extensive research in Maine sources apparently was not supplemented by readings in more general sources in economic, labor, architectural, or political history. The volume makes no attempt to analyze the impact of changing technology on labor, management, government, or society, and there is no analysis as to how the limestone lobby triumphed over the granite lobby, or why politicians so quickly accepted innovations in the construction of public buildings.

CHARLES F. CARROLL
University of Lowell

LYNN M. ALPERIN. *Custodians of the Coast: History of the United States Army Engineers at Galveston*. Galveston, Tex.: Galveston District, United States Army Corps of Engineers. 1977. Pp. xi, 318.

From the time of exploration the Gulf coast of Texas posed special problems for would-be navigators. A glance at the map showed a long coastline broken by inlets and rivers and inspired dreams of easy maritime development. But those dreams vanished with the first experience. The inlets which looked so inviting on paper were blocked by shifting sandbars, and the rivers which seemingly opened the interior to trade were temperamental, carrying too much water or too little depending on the season. In the absence of better means of transportation the early settlers continued to dream of water transportation and to plan

means of coping with their erratic waterways, but only with the advent of improved technology and the United States Army Corps of Engineers did the dreams become reality.

This book traces the achievements of the Engineers from their first appearance on the Texas coast to the present. They first established themselves at Galveston, the best natural harbor in the state, and made it a deep-water port. Then they diagnosed and solved the problems of navigation at the Sabine, Buffalo Bayou, Aransas Pass, Brazos de Santiago, and other points. Often they used innovative and experimental methods, and along the way they developed the Gulf Intracoastal Waterway and coped with the hazards of Gulf coast weather.

The story is one of man's triumph over nature, but that too has posed problems for a society conscious of its environment. The achievements of the Engineers have expedited the development of the petroleum and allied industries, which in turn have boosted the economic growth of Texas. But those same achievements have altered the face of the land. The Engineers are not insensitive to the problem, and a last chapter deals with the problem of reconciling progress with ecology.

This book was written for and published by the Corps of Engineers and has the obvious shortcomings of such publications. That is, the approach is generally uncritical. Even so, it is a straightforward, well-documented account that will be of interest to engineers, businessmen, mariners, and ecologists.

MARILYN MCADAMS SIBLEY
Houston Baptist University

SH. A. BOGINA. *Immigrantskoe naselenie SShA, 1865-1900 gg.* [The Immigrant Population of the U.S.A., 1865-1900]. Summary in English. Leningrad: Izdatel'stvo "Nauka," Leningradskoe Otdelenie. 1976. Pp. 273. 1 r. 43 k.

In 1969 N. N. Bolkhovitinov reviewed in this journal the status of the study of United States history in the Soviet Union (*AHR*, 74[1969]: 1221-42). Bolkhovitinov emphasizes that American studies is a relatively new field with a common set of features: a Marxist method of analysis; a focus on the real makers of history—workers, farmers, and artisans; attention to the struggle of blacks for liberation; and emphasis on the reactionary ruling circles in domestic and foreign policy. Since 1967 Soviet publications have retained this focus but have grown considerably with respect to quantity and range of topics, including philosophy, the military-industrial complex, the 1960s, and historiography.

Sh. A. Bogina's study, a sequel to her book on

immigration into the United States from 1850 to 1865 which focuses on Irish and German immigrants (*Immigratsiia v SShA nakanune i v period grazhdanskoi voyny, 1850–1865* [1965]) reflects the pattern in Soviet studies. As in her earlier study, Bogina applies a Marxist perspective, but it is not excessive as she takes a rather straightforward approach, concentrating on German, Italian, and Scandinavian immigrants in separate chapters and then examining Irish, British, Chinese, and French Canadians in another chapter. Bogina analyzes each group with respect to why the immigrants left their countries, the occupations they entered in the United States, the process of assimilation they experienced, and the contributions they made to American society. The differences between German and Italian immigrants, an example of the shift in immigration from Western Europe to Southern and Eastern Europe, is successfully minimized by Bogina who rejects the argument that the “new immigrants” differed considerably with respect to their social background although she does recognize cultural differences. Bogina also continues to rely on published secondary American sources through 1970, omitting important studies by Josef Barton and Irving Howe.

Bogina’s central thesis is that the United States under the impact of industrialization changed more than the nature of the immigrants coming into the United States. The distance between Americans and the “new immigrants” was considerably greater with respect to economic, social, and cultural developments. Consequently, assimilation, with economic pressures being the most important force, was more difficult, especially in the face of ethnic and racial discrimination. Bogina touches on the nativist response to the immigrants but she does not give sufficient attention to John Higham’s thesis on status rivalries including ethnic competition for status and power. In a concluding chapter Bogina reviews the American historiography on immigration, noting the emergence of the “melting pot” thesis and its earliest rival, cultural pluralism. Bogina suggests that the pressures for integration predominate over ethnocentrism, but she slights the issue of ethnic diversity in American society, especially theories such as Higham’s pluralistic integration.

Although Bogina’s study offers very little new data or interpretations for the American specialist, it is valuable for Soviet scholars. This reviewer hopes that Bogina will turn to the East European Jews, who are ignored in this study, and build her next book in part on primary sources not readily accessible to American scholars.

THOMAS R. MADDUX
California State University,
Northridge

JAMES B. GILBERT. *Work without Salvation: America’s Intellectuals and Industrial Alienation, 1880–1910*. Baltimore: Johns Hopkins University Press. 1977. Pp. xv, 240. \$14.00.

The changing nature of daily work is a central aspect of America’s social history. An expansive and elaborate treatment of the facts of work illuminates how people live, raise children, get educated, get along with each other, play, and die. Freud said that finally there is meaning only in love and work.

The distance from the Calvinist concept of the calling to sabotage on an assembly line is measured in terms of profound social and economic change. How does one most usefully take this measure? In *Work without Salvation: America’s Intellectuals and Industrial Alienation, 1880–1910*, James B. Gilbert selected a pivotal phase of the history of work. He examined the ideas about work held by some of the writers and thinkers of the time, rather than analyze the work process itself. As a consequence, the point and feel of changing work are somewhat sublimated, and are kept at a distance from the reader. Nonetheless, the attitudes and opinions of the intellectuals that are displayed in this book are interesting and probably reflect deeper and wider social convictions clearly that were under stress.

The rise to dominance of the corporation was the key to the reorganization of work into meaningless human activity. This led to a hazy, romantic celebration of preindustrial craft labor, on the one hand, and to an equally blind advocacy of the “work ethic,” on the other. Both attitudes searched for an evasion, not a solution, to the fact that work was actually becoming trivialized and that people did not like it. What was at stake was what conservatives of every stripe called “the social order,” that is, the accustomed relationships and patterns of social organization that provided benefits to the already privileged.

Gilbert makes William James the keystone of this study. James had a divided mind on the subject because he yearned after individualism while he was suspicious of traditionalism. As Gilbert puts James’ genteel international cultural viewpoint: “While institutions, dogmatism, and the new industrial culture threatened traditional concepts, this emphasis upon distinctions, creativity, and individualism made James particularly aware of the intellectual consequences of the crisis of work” (p. 187). But after two chapters that analyze aspects of James’ intellectual system, Gilbert is obliged to conclude that “James ironically demonstrated that the individuality and variety he so cherished had little operational value in the advancing industrial culture of his time . . .” (pp.

210-11). Indeed, the relationship of James to the rest of the book is not clear, except that he was alienated from the emerging industrial world.

There is a concreteness about daily work that suffers under abstraction and distancing. Perhaps this is why the immediacy of the personal statements in Studs Terkel's *Working* is convincing.

LOREN BARITZ
State University of New York

MICHAEL H. EBNER and EUGENE M. TOBIN, editors.
The Age of Urban Reform: New Perspectives on the Progressive Era. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. viii, 213. Cloth \$12.95, paper \$7.95.

The ten essays in this book deal with the "good government" aspect of progressivism in Houston, Chicago, Seattle, New York City, Passaic, Los Angeles, Philadelphia, Atlanta, Jersey City, and Chelsea, Massachusetts. Each essay is by a different author, and, like most such collections, the quality of the essays varies. Augustus Cerillo's subtle analysis of the relationship between the ideology and achievements of a number of reformers shows them to be people with a realistic understanding of power, who, if they did not achieve everything they aimed at, were not naive idealists. Martin G. Schiesl makes the important point that we must be careful not to react to progressive hyperbole by over-romanticizing political bosses. The "progressive" administrators of Los Angeles learned the realities of politics and built a machine of their own but one which was less exploitive and more alert to social welfare needs of the citizenry than was the machine they replaced. Wayne J. Urban explicates the relationship between "progressive" educational reform and other issues of local and state politics in Atlanta. He concludes that "efficiency" in administration was a more important goal than a Deweyan model of what education ought to be.

The authors share an ambiguity, which could at least be faced directly, about words such as "elite," "democratic," "humane," and of course "progressive." A certain amount of fuzziness about these words is perhaps unavoidable, but the authors do not always seem aware of the problems inherent in the terms.

A book of essays by different authors needs a common theme, either in method or content. These essays are all about cities and reform, 1890-1920. That is not enough to provide a common bond. There are some implications of a "new" urban history, for the book includes numbers and tables. These are, beside being printed in near microprint, unanalysed and mentioned in the text

only in passing. The editors' introduction does not succeed in finding a common thread, and there is no concluding essay.

The chapters on Chelsea, Houston, Chicago, and Seattle implicitly have a common theme, though the authors do not seem aware of it. "Good government" forces in all four cities were, as Hofstadter and others long ago suggested, all ethnic or class conservatives, feeling that lesser breeds, however defined, should be kept from civic power. None of the authors makes much of this point, nor indeed of any other.

The Age of Urban Reform, then, contains some articles with useful points, but does not add up to a coherent book.

DANIEL LEVINE
Bowdoin College

JOHN M. ALLSWANG. *Bosses, Machines, and Urban Voters: An American Symbiosis.* (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. ix, 157. Cloth \$9.95, paper \$5.95.

This is a book about an American original, the city boss and his machine. From Lord Bryce to Leo Hershkowitz, he has either been damned or white-washed. As the late Richard Daley said, "They have vilified me, they have crucified me, yes they have even criticized me." John Allswang's study, modest in size but meaty in content, walks a neat moral tightrope between "good" and "bad," recognizing that in many political situations value judgments merely clutter meaning or are irrelevant. His approach is both judicious and imaginative and, to employ a tired cliché, is a fine contribution to the literature.

He begins with a discussion of the shifting interpretations of the city boss, dealing with such familiar boss watchers as Bryce, Ostrogorski, Plunkitt, Steffens, Orth, Salter, Merton, Merriam, and Gosnell, an excellent chapter flawed only by the omission of the contributions of professional urban historians writing in the 1960s and 1970s. He then analyzes, compares, and contrasts five city bosses of the nineteenth and twentieth centuries, William Tweed and Charles Murphy of New York, and William Hale Thompson, Anton Cermak, and Richard Daley of Chicago. Stitching these chapters together is the question: how do you account for the phenomenon of the city boss? He finds his answer in the political bedrock of the vote—who voted and why, or more precisely, the vote of the ethnic urban masses. Ethnicity was (and still is) the key to the city boss in America. Buttressing this theme is a quantitative analysis of the demographic patterns in the cities of each of the bosses, executed deftly without jarring the flow of prose.

In a uniformly good book, I found the chapter on Daley outstanding, especially his treatment of Daley's relationship with the blacks. It is an exercise in balanced judgment, refreshing in comparison to the thump in the groin delivered by the Chicago journalist, Mike Royko.

Allswang leaves us with another question to be pondered. We may wince at the waste, graft, and inefficiency of the machine, but if we consider the plight of the native and immigrant poor, the services offered by a Tweed or a Daley, the fragmented structure of local and state government, rapid urbanization, technological innovations, the goals of the machine politician, and the ideals of the urban reformer, what, indeed, were the viable alternatives to the city boss and his machine?

ALEXANDER B. CALLOW, JR.
University of California,
Santa Barbara

JEFFREY J. CROW and ROBERT F. DURDEN. *Maverick Republican in the Old North State: A Political Biography of Daniel L. Russell*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1977. Pp. xv, 202. \$14.95.

Daniel L. Russell was the only North Carolina Republican governor elected between Reconstruction and 1970. His election in 1896 at the head of a fusion of Populists and Republicans represented a significant tradition of dissent in the post-Civil War South. Recent biographies have established the basic thesis of the book, that there was an alternative to the racial and economic orthodoxy of Redeemer Democrats, and that the South generally rejected such an alternative because of the conservatism of southern culture, political opportunism, and the pre-eminence of race in southern politics.

Russell inherited from his affluent planter family racial paternalism, social arrogance, and Whig politics. He ardently defended the right of blacks to vote so long as they elected whites to lead them. Democrats were so distasteful to him that the only shot he is recorded as firing in defense of the Confederacy was aimed at a Confederate officer who administered the draft in North Carolina. As governor of North Carolina, he allied more closely with the Populists over the issue of railroad regulation than with his own Republican Party.

This brief biography is not a comprehensive study of the man; as the subtitle indicates, it is a political biography. Even evaluated entirely by that standard, the biography has gaps. Chapter three moves Russell from 1878 to 1896 in a thin analysis of only eighteen pages. It does not explain adequately the complex fusion of Republicans and

Populists or Russell's growing racism. As a result, his ambivalence on race seems more related to political opportunism than to "a spirit of old Republican principle and Whiggish paternalism. . ." (p. 62). His later handling of North Carolina railroad bonds, which he manipulated to his personal financial gain, creates the same problem. Jeffrey Crow and Robert Durden attribute his conduct to Whiggish-Republican philosophical revulsion against repudiation of state debts. A more cynical reader might conclude that Russell acted primarily to make a great deal of money in lawyer's fees by representing a New York banker who was the major mortgage holder.

Although the book needs more tough-minded analysis, it makes a contribution as a troubling probe of late nineteenth-century southern politics. Despite his racial paternalism, Russell defended black participation in government until "Red Shirt" terrorism and Democratic fraud denied Negroes the ballot. Perhaps Russell accurately named the coconspirator in this tragedy when he urged Congress not to abandon the Negro. His pleas went unheeded creating the paradox of a southern Republican who cared more for the rights of blacks than did his nonsouthern colleagues in Congress.

WAYNE FLYNT
Auburn University

HERBERT F. MARGULIES. *Senator Lenroot of Wisconsin: A Political Biography, 1900-1929*. Columbia: University of Missouri Press. 1977. Pp. x, 432. \$21.00.

In the preface to this detailed and carefully documented political biography, a briefer version of an earlier and more comprehensive unpublished study on deposit with the Lenroot papers at the State Historical Society of Wisconsin and at the Library of Congress, the author says his purpose "is to illuminate and draw lessons from Lenroot's public and political career" (p. x). The chief lesson that emerges from his biography is that political factionalism rather than ideology determined Lenroot's course from an ardent associate of Robert M. LaFollette in Wisconsin to an administration ally in the era of Republican ascendancy.

Herbert F. Margulies devotes his attention to the years of Lenroot's legislative service. Elected to the Wisconsin Assembly in 1900, Lenroot served in a legislative arena until March 1929 when his tenure ended following his defeat for renomination in the 1928 Wisconsin senatorial primary. Coolidge appointed him to a vacancy on the Court of Customs Appeals in February 1929. Though Lenroot had a further career, Margulies devotes scant attention to it, preferring, correctly, to focus on the

three decades when Lenroot made a useful contribution to both state and national politics. From 1901 through 1906 he was LaFollette's chief lieutenant in the Wisconsin Assembly, twice serving as speaker. Elected to the House in 1908 he was a radical insurgent in the battle against Cannonism, seeking to remove the speaker, Joseph G. Cannon, as well as to minimize his power. He served for a decade in the House of Representatives where he established himself as one of its ablest members. Elected to the Senate in 1918, Lenroot spent the final decade of his legislative career as a useful and respected member of this body. In 1920 he was the Republican leaders' choice for the vice-presidential nomination at the Chicago convention. The delegates, however, defied the wishes of his senatorial colleagues and selected Calvin Coolidge, thereby depriving Lenroot of the presidency while at the same time entitling him to a footnote in the presidential annals. Almost two-thirds of his legislative career was spent in bodies other than the United States Senate. Thus the biography is mistitled; more than half the volume is devoted to Lenroot's earlier career.

By any standard Lenroot rates high as a legislator. He was a skilled draftsman, an able parliamentarian, and an exceedingly effective debater. But he almost always was somebody else's man; Lenroot rarely dominated the legislative scene in leading a fight or championing a cause. He was invariably a chief lieutenant serving in someone else's army, playing an important role in their battles. First, of course, it was Robert M. LaFollette in Wisconsin. When Lenroot entered the House he played an important role in the insurgency movement. After breaking with LaFollette in 1912, a break that was exacerbated in 1917, Lenroot became the chief associate of James R. Mann, the House minority leader. When he entered the Senate he quickly became Lodge's chief lieutenant and associate, seeking mild reservations in the League fight. In the 1920s, now a regular Republican, he led the Senate fight for the Agricultural Credits Act of 1923 and was a key administration supporter in that chamber.

Margulies insists that Lenroot was consistent, that his views did not change, that the values he espoused as a Wisconsin Progressive he endorsed for the rest of his career. Since his Progressivism did not encompass economic issues, there is some validity to Margulies claim, but his argument will not convince all of his readers. Aside from this point, Margulies faced another difficulty that he did not fully resolve. While he provides excellent discussions, some of the best brief examinations available of the topics Lenroot was involved with, he often loses sight of his subject for pages on end. This is a dilemma that every biographer faces, and

it is particularly difficult when the subject is always cast in a supporting role.

RICHARD LOWITT
Iowa State University

MICHAEL D. MARCACCIO. *The Hapgoods: Three Earnest Brothers*. Charlottesville: University Press of Virginia. 1977. Pp. xii, 259. \$15.00.

This study of Norman, Hutchins, and William Hapgood is a valuable addition to the autobiographies of two of the brothers. There were striking differences in temperament and personality and not much brotherly love in the Hapgood clan. Yet Michael Marcaccio argues that they acquired from their idealistic businessman father an earnestness which bound them together as "intellectual and moral brothers." Perhaps.

Norman vaulted from drama critic on the *New York Commercial Advertiser* to editor of *Collier's Weekly* in 1903. A cautious progressive, he shaped that magazine into an influential reform journal. He was at his best in the Ballinger-Pinchot affair, a controversy he helped create by publishing Louis Glavis' charges. Usually, however, Norman Hapgood avoided direct confrontations. During the 1912 campaign he informed his father that he would "support Wilson energetically while still giving T. R. his due." Hapgood aimed at political detachment on *Harper's Weekly*, his next journalistic venture, but he ended up giving unwavering support to the Wilson administration. He was appropriately rewarded with a diplomatic appointment after *Harper's Weekly* folded in 1916, and later he found employment with the Hearst press. Finley Peter Dunne pegged him correctly in 1905: "Th' Homeeric Legend an' Graft; Its Cause an' Effect; Are They th' Same? Yes and No," by Norman Slapgood."

Nonconformist Hutchins Hapgood also started out as a newspaper reporter. His preferred beats were the Bowery, which he explored with Frances Willard's wayward nephew, and the Lower East Side where his cicerone was fellow reporter Abraham Cahan. After publishing *The Spirit of the Ghetto* and *Types from City Streets*, Hutchins Hapgood wrote about labor, radicals, Greenwich Village life, and his own marriage. Emotional problems paralyzed him as a writer for nearly two decades. In his fine autobiography, *A Victorian in the Modern World*, he referred to himself during the long fallow period as being "rudderless, like the rest of the world." Marcaccio offers several explanations for the malaise and tentatively concludes that it was "perhaps related to his not performing up to his father's norms and a consequent feeling of deficiency. This was the source of much of his frustration, although he was only partly conscious of it."

Neither Norman nor Hutchins expected William to amount to much, and Marcaccio considers him the least important. Yet the self-confident William served an apprenticeship in the wholesale grocery business and then ran his own small canning company. Showing a deeper commitment to reform and more earnestness than his brothers, he permitted workers to share company profits, allowed a workers' council to set pay scales, and encouraged employees to purchase a majority of the common stock. Widely acclaimed in the 1920s as a model experiment in industrial democracy, Columbia Conserve Company survived the Depression and stayed afloat until 1953. Unfortunately, William's saga is confined to a single chapter. It deserves to be set in the context of wartime industrial relations, other experiments in welfare capitalism, and a fuller analysis of the views of stockholders Norman and Hutchins Hapgood, both of whom wrote extensively on labor and industry.

The author has a firm grip on Norman Hapgood, and his discussion of journalistic combat in the early twentieth century is first-rate. Few readers will disagree with his verdict that the Hapgoods were significant but by no means pivotal figures in their generation.

LOUISE C. WADE
University of Oregon

JEFFREY J. SAFFORD. *Wilsonian Maritime Diplomacy, 1913-1921*. New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xii, 282. \$16.50.

This book fills a gap in early twentieth-century United States history by tracing the formulation and implementation of merchant marine policies under Woodrow Wilson's two administrations. In addition, it relates maritime concerns to administration activities in the fields of finance, economics, and diplomacy. The bulk of the story comes after the passage of the Shipping Act of 1916, which providentially allowed the government to assemble and operate a merchant fleet in the event of "national emergency." That legislation enabled the creation of the Emergency Fleet Corporation and the United States Shipping Board when the United States entered World War I. Thereafter, shipping formed an important American contribution to Allied victory in 1918. At the same time, shipping controversies occasioned severe strains between the United States and the Allies during the war and its aftermath, as policy-makers on both sides of the Atlantic eyed each other suspiciously and girded for anticipated postwar economic conflict. Jeffrey J. Safford depicts and interprets these events with clarity, authority, and insight, especially regarding the role of the war-

time Shipping Board head, Edward N. Hurley. In all, *Wilsonian Maritime Diplomacy* is a solid, serviceable work, which will be useful to students of maritime history and World War I.

Unfortunately, two kinds of flaws detract in varying ways from the book's overall merit. The more serious flaw is failure to show that maritime concerns formed anything more than one aspect, and hardly a major one, of Wilsonian diplomacy. Aside from its obviously critical part in the war effort of 1917-18, merchant shipping rarely seems to have occupied much of the attention of Wilson or his chief advisors. Likewise, much of the political squabbling over government-sponsored mercantile expansion before 1916 was far less important than the actions taken afterward. Those conflicts, therefore, scarcely merit the detailed treatment given in the book's first eighty-odd pages. Moreover, throughout the book Safford recounts too much familiar material in unsuccessful efforts to ascribe important maritime aspects to a variety of Wilsonian policies. On a different level, the book suffers from irritatingly frequent factual mistakes, typographical errors, and stylistic stumblings. Both kinds of shortcomings mar the generally high quality of a book based upon sober judgment and extensive research.

JOHN MILTON COOPER, JR.
University of Wisconsin,
Madison

DAVID GORDON NIELSON. *Black Ethos: Northern Urban Negro Life and Thought, 1890-1930*. (Contributions in Afro-American and African Studies, number 29.) Westport, Conn.: Greenwood Press. 1977. Pp. xviii, 248. \$14.95.

David Nielson has given us in *Black Ethos* an exhaustive, and in some ways exhausting, study of black urban life in the early twentieth century. The volume is exhaustive in dealing with most aspects of black life—class structure, migration, artistic activities, and color consciousness—to name but a few topics. Nielson, of course, does not spread his concern evenly over these subjects, for that would be impossible in a book of less than two hundred fifty pages. Yet he does have something of interest—a description, a comment, an analysis—on most topics in Afro-American history. Nielson draws on a variety of printed sources, including novels, poetry, autobiographies, monographs, and the contemporary press. The wide variety of subjects, along with the author's tendency to let the reader know he has researched his subject thoroughly, occasionally makes for the exhausting reading I noted earlier.

It may seem odd then that my major criticism of *Black Ethos* lies in the author's failure to make use of yet another type of source material. Why criticize Nielson for not looking even further when he has looked far indeed into the sources of Afro-American history? The criticism comes largely out of the author's claim that he has analyzed the views of "ordinary black Americans." If an author is to make such a claim, he must go beyond the usual printed or manuscript sources most of us use. Yet in developing his interpretation of the "ordinary black American" and his beliefs, Nielson draws on the same sources used by scholars who have explored black organizations of the era, the Harlem Renaissance, and black writers and artists, and by those who have written biographies of black leaders of the period. Nielson, like the rest of us, draws on the pages of *Opportunity* and the *New York Age*, on the insights of Langston Hughes and Claude McKay, and the studies done by such historians as Richard Dalfiume and Gilbert Osofsky. Nevertheless, these sources, and the many others he draws upon to good advantage, can only be reflections, perhaps distant ones at that, of the "ordinary black American" Nielson wishes to depict.

Two sources quickly come to mind in the search for an understanding of the mass of blacks in the early twentieth century: black music and oral history. During the 1920s blacks purchased hundreds of thousands of so-called race records, the lyrics of which cover practically every phase of black life, North and South. Twenty years ago Paul Oliver published an insightful study of these recordings, *Blues Fell This Morning*, and over ten years ago Charles Keil gave us his *Urban Blues*. There is much that Nielson could have used in these volumes and even more in the music itself. Finally, the past decade has seen an explosion of oral history projects, a few of which are concerned with the lives of "average" blacks. Very little of this material has found its way into print. However, on the basis of a few examples (see Peter Gottlieb, "Migration and Jobs: The New Black Workers in Pittsburgh, 1916-1930," *Western Pennsylvania Historical Magazine* [1978]), the careful analysis of these accounts holds promise of giving us insights into vital aspects of Afro-American history, aspects which have been previously shielded from view.

Nielson has given us a tightly written, thoughtful book, which will, I hope, be widely read. For all its virtues, however, it is not the study he hoped it would be. If we are to understand the "ordinary black American" in the Jim Crow era, we must go beyond the writings of the black elite and do our best to contact the black masses themselves.

EUGENE LEVY
Carnegie-Mellon University

WILLIAM H. HARRIS. *Keeping the Faith: A. Philip Randolph, Milton P. Webster, and the Brotherhood of Sleeping Car Porters, 1925-37*. (Blacks in the New World) Urbana: University of Illinois Press. 1977. Pp. xiv, 252. \$12.50.

This book appears at an interesting moment. Its membership having dwindled from a claimed 18,000 in June 1949 (with established divisions in 117 U.S. and Canadian railroad centers and bargaining agreements with Pullman and thirty-nine "class A" railroads) to about 1,000 members in 1977, the Brotherhood of Sleeping Car Porters voted on February 28, 1978 to affiliate with the Brotherhood of Airline and Railway Clerks. Thus the best-known black union has disappeared about the same time that this study of its founding and early history makes its appearance. Many of the basic facts covered in this study have already been published in Brailsford R. Brazear's *Brotherhood of Sleeping Car Porters* (1946), but William H. Harris has deepened our understanding as a result of his research in a wide range of archival materials, especially the valuable correspondence of the Brotherhood in the manuscript collections of the Chicago Historical Society.

Harris describes the founding of the Brotherhood in 1925 when these terribly exploited black workers had to face not only the bitterly antiunion Pullman Company but a labor movement which was generally anti-Negro. In its efforts to build a viable movement the Brotherhood, Harris makes abundantly clear, had to fight not only the powerful Pullman Company, the largest single employer of blacks at that time, but sections of the black community in alliance with the corporation, and the racism of organized labor. In overcoming these seemingly insuperable obstacles, Harris points out, the porters had the charismatic leadership of A. Philip Randolph and the valuable organizing abilities of Milton P. Webster. Harris is especially good in pointing up the contributions of Webster, a former porter himself, who at first was unable to communicate his ideas effectively in writing. But he had numerous contacts among the porters which Randolph, an outsider, lacked, and he was able to lend considerable strength to Randolph's ably written appeals by behind-the-scenes operations without which the fledgling union might have succumbed.

Harris traces in detail the tactics and strategy of these black labor leaders, including such local union officials as C. L. Dellums, Ashley L. Totten, Benjamin Smith, and E. J. Bradley, a number of them barely educated, in winning the support of administrative agencies, sections of the AFL leadership, the courts, and public opinion, and in soliciting aid from civil rights groups and foundations.

He throws new light on the important role played by the American Fund for Public Service (the so-called Garland Fund) in enabling the Brotherhood to keep its head above water. Strangely, however, in these days of increasing interest in the role of women and the labor movement, Harris is silent on the major contribution of the porters' wives and women relatives organized in the Colored Women's Economic Council. The Council formed women's auxiliaries in various cities, which staged rallies, bazaars, picnics, boat rides, theater benefits, and other types of fund-raising socials. Of particular importance was the help the auxiliaries gave to porters' families who had suffered because of Pullman's dismissals of known union members.

This silence is probably the result of Harris' tendency to concentrate on the union's leadership. Another limitation of the study is a failure to examine critically Randolph's practice of retreating from advanced positions during his union leadership, a tendency he also demonstrated as editor of *The Messenger*. Although Harris notes that Randolph was sharply criticized for having called off the strike in 1928 after the membership by a vote of 6,053 to 17 had voted to walk out to force Pullman to negotiate, he fails to develop the theme and leaves the reader confused as to what course should logically have been pursued.

All this, however, should not obscure the fact that Harris' book contains a wealth of insights and basic data which successfully places the history of this remarkable union in its time. The book is a welcome addition to the historiography of the black working class. It would be useful if the series in which this volume appears, edited by August Meier, included a follow-up study carrying the history of the Brotherhood of Sleeping Car Porters from 1937 to its merger with the Clerks' Union.

PHILIP S. FONER
Lincoln University

RAYMOND GAVINS. *The Perils and Prospects of Southern Black Leadership: Gordon Blaine Hancock, 1884-1970*. Durham, N.C.: Duke University Press. 1977. Pp. x, 221. \$11.75.

Southern black leaders—"race" men and women—during the period between the death of Booker T. Washington and the emergence of Martin Luther King, Jr. faced numerous difficulties. They operated in a world of limited options, and their dual role as spokesmen for blacks and liaisons to whites required them to walk numerous tightropes. In this insightful and well-balanced biography Raymond Gavins has enlarged our understanding of what talented, proud, and race-conscious blacks encountered when they assumed leadership positions within a caste society.

Gordon Blaine Hancock was a case in point. Born in 1884 in Greenwood County, South Carolina, Hancock received his education at Benedict College, a South Carolina school for Negroes that had been founded during Reconstruction by the American Baptist Home Mission Society, and later at Colgate and Harvard. After earning an M.A. in sociology from Harvard in 1921 Hancock passed up more lucrative opportunities in the North and returned south to teach at Virginia Union University in Richmond, an institution oriented toward the black middle class and an important center of black intellectual life. He also became pastor of the Moore Street Baptist Church in the city.

By the 1930s Hancock was an influential leader with a national reputation. In addition to his work at the college and the church, he was the author of a syndicated weekly column that appeared in more than one hundred black newspapers. On most issues he stood squarely between "old guard" black leaders who, with undue optimism, continued to advance the racial self-help philosophy symbolized by Booker T. Washington and "young radicals" who advocated more militant protests against discrimination.

Because he accurately pointed out the economic conditions faced by most blacks in bleaker terms than did the diehard Washingtonians yet continued to prescribe the Washingtonian virtues of hard work, solidarity, and thrift, students at Virginia Union called Hancock the "Gloomy Dean." To be sure, his message was not cheerful, but in characterizing him as a negativist, younger black contemporaries, suggests Gavins, frequently overlooked Hancock's shrewd grasp of realities, his ability to communicate with a broad audience, and his underlying opposition to segregation. Far from accepting "separate-but-equal," Hancock viewed it as an unjust arrangement, but a temporary one under which blacks could achieve economic and educational advancements that in the future would enable them to eliminate Jim Crow. Through his sympathetic detachment and understanding of the subtleties of Hancock's milieu, Gavins portrays a man who, more than most black leaders before or since, came close to reconciling the principal issue—separatism versus integration—that has been a center of controversy within the Afro-American community for two hundred years.

Gavins has also done an excellent job of probing into Hancock's vacillating relations with white southern liberals. Since "interracial cooperation" was crucial to his overall program, Hancock sought alliances with southern whites. He managed two notable successes in this effort. In 1928, in response to the criticisms of Hancock and other blacks against white indifference to the problems

faced by Richmond's Negro population, the city appointed a Negro Welfare Survey Committee. The interracial committee published a frank report demonstrating that black Richmonders, though the most needy group in the city, received the fewest social services and lacked sufficient opportunities to overcome what was described as their most pressing burden: economic deprivation. "If it told Negroes what they already knew," writes Gavins, "the survey presented information heretofore evaded by whites" (p. 49). Hancock's second successful interracial effort began in 1942 when, in the midst of wartime racial anxieties, he brought together black leaders in Durham, North Carolina who issued a statement of grievances, which, though it stopped short of calling for an immediate end to segregation, made it clear that blacks would accept nothing less than far-reaching changes in the racial status quo. Says Gavins of the Durham declaration: "Never before in southern history had black people made such a comprehensive and lucid declaration of what they expected from white people" (p. 127). The Durham manifesto prompted subsequent meetings of whites and blacks, which in 1944 resulted in the formation of the Southern Regional Council.

Despite such accomplishments, Hancock's interracial approach ultimately failed. The flaw, Gavins correctly points out, was that, unlike Hancock, southern liberals seldom envisioned a desegregated South. Yet a few did, and it was this perspective, which emphasized regional patriotism and a missionary desire to redeem the South, that Hancock shared with them, and which often put him at odds with other blacks and northern liberals. All students of the modern South should be grateful to Raymond Gavins for illuminating the life of a complex man who, if he did not live up to the expectations of civil rights crusaders, was a far cry from the "Uncle Tom" stereotype that too often has been unfairly applied to southern black leaders of his generation.

MORTON SOSNA

National Endowment for the Humanities

TETSUDEN KASHIMA. *Buddhism in America: The Social Organization of an Ethnic Religious Institution*. (Contributions in Sociology, number 26.) Westport, Conn.: Greenwood Press. 1977. Pp. xvii, 272. \$17.50.

Scholarly studies of Japanese Americans (*Nikkei*) have focused largely on political and economic themes—particularly those related to their World War II incarceration—but the religious dimension has been neglected. Religions in Japan have been well covered, but their transplantation in America

by immigrants and their descendants have not received scholarly attention until the appearance of this book.

Tetsuden Kashima begins with a brief survey of Japanese religions and the Buddhist sect predominant among *Nikkei* today, the *Jodo Shinshu*. The adaptations necessary to survive among the acculturated second (*Nisei*) and third generation (*Sansei*) descendants of the immigrants (*Issei*) in America included the creation of the YMWBA (Young Men and Women's Buddhist Association) to compete directly with the YMCA and YWCA. Structural modifications were not without fundamental doctrinal distortions due to differences in concepts such as Nirvana and God and translation and communication problems between Buddhist ministers from Japan and their *Nikkei* congregations.

A strong and pervasive theme is the continuing role of Buddhist churches as local and regional centers of ethnic social and cultural solidarity. The establishment of the first Buddhist Church in America (San Francisco, 1898) coincided with the arrival of large numbers of *Issei* on the Pacific Coast. By 1913 California had enacted laws prohibiting their owning or leasing land, and in 1924 Congress stopped further immigration. Long before their World War II ordeal, *Nikkei*—aliens and citizens alike—were strangers in a hostile land, and the Buddhist churches were often their sole centers for social activities. This function continued during World War II, but Kashima's cursory treatment of the concentration-camp period is disappointing.

After the release of *Nikkei* the Buddhist churches were crucial to their return and eventual recovery, for the long-awaited homecoming became yet another episode in the nightmare which began after Pearl Harbor. Homes, farms, and businesses had been lost during the war; thus Buddhist churches became crowded tenements until families found employment and living quarters. Kashima's brief section on the Buddhist churches' role as refuges for returnees whets the appetite for more research on the decade following the war.

The study also includes recent issues which find the American Buddhist churches confronted with problems similar to other religious institutions: declining attendance, failure to attract young members, doctrinal disputes between *Nikkei* ministers and those from Japan, and the rapidly changing and complex social fabric of contemporary America (e.g., the role of women and non-*Nikkei*).

Kashima comes from a family of Buddhist ministers, but his "insider's" account avoids personal pronouns and parochial preferences. He may be challenged for omissions and superficiality, but this book is a much-needed gap-filler. As such, it

buttresses the growing body of recent studies on Japanese Americans with a competent general survey likely to provoke and inspire further research and publications.

DONALD TERUO HATA, JR.
California State University,
Dominguez Hills

JÜRGEN KOCKA. *Angestellte zwischen Faschismus und Demokratie. Zur politischen Sozialgeschichte der Angestellten: USA 1890-1940 im internationalen Vergleich.* (Kritische Studien zur Geschichtswissenschaft, number 25.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 556. DM 92.

In this heavily revised and expanded *Habilitationsschrift* Jürgen Kocka has set himself ambitious and important goals which include a comparison of the development of the lower and middle ranks of organized American and German white-collar workers and a cautionary redefinition of Fascism. His aim, in essence, is to examine the reasons for the lack of support from white-collar workers (*Angestellten*) for a fascist movement in the U.S. and the contrary situation in Germany. This analysis makes a contribution to the continuing discussions on the relationship between fascism and capitalism which concerns a growing number of scholars, especially in Europe.

Kocka notes that the historical experience of the growth of fascism in Germany, as related to a major capitalist crisis and the protest potential of the lower middle class on the Right, has led many scholars to certain generalized assumptions about the nature of the relationship among capitalism, the lower middle class, and fascism, especially the assumption that these *Angestellten* almost automatically support fascism when threatened as in the 1930s. Kocka's aim is to throw these assumptions open to question, since they suggest that similar conditions in the U.S. should produce the potential for a similar right-wing protest. The body of the book attempts to find this specifically white-collar, lower-middle-class, rightist protest potential in the United States. Kocka's conclusion is that it hardly existed (p. 13).

Thus there is at least a partial explanation for the fact that under similar conditions one capitalist-bourgeois economic and social order became "perverted" into fascism and another did not. This is an important point, but hardly as new as Kocka seems to think. (He frankly admits that the causes of the lack of an important domestic U.S. fascist movement cannot be found only in an analysis of the development of the organized white-collar workers.)

Furthermore, he appears to contradict himself

by noting that these other causes would have prevented an equivalent influence of a fascist movement in the U.S., even if a class-oriented rightist protest potential similar to that in Germany had existed (p. 297). After the tremendous amount of work that went into this project (550 pages of closely printed text, notes, and charts), the thesis that the U.S. did not experience such a protest potential seems rather modest. Indeed it is elsewhere that one must look for the main point of the book: Kocka believes the results of his analysis have successfully countered the traditional arguments on the subject which he examines in an early chapter. It is here that the interest of the book lies.

Kocka comes to the conclusion that 1.) the American white-collar workers did not react to the crisis of the 1930s in a class-oriented way which would have set the lower middle class against the working class; 2.) they did not offer a potential for a radical Right or fascist movement; and 3.) the differences between the working-class and white-collar workers were less important for the creation of contrasting political, social, legal, economic, and psychological dimensions in the U.S. than in Germany. Since Kocka believes these conclusions run counter to what he calls the "traditional" arguments, he notes that in the future scholars will have to be more cautious in considering the relationship between crisis-ridden capitalism and lower-middle-class reaction with regard to fascist movements in general. The implication here is that there can be no general theory of fascism but only a theoretical structure to fit each national or regional experience. Hopefully this idea will spur the fascism debate further onto the territory of empirical studies to support the theoretical structures of explanation.

Finally, Kocka makes the important point that the differences between the development of his subject in the U.S. and Germany would remain in many ways undiscovered without the application of a comparative international perspective and analysis. If one must remain somewhat sceptical regarding some of Kocka's claims of originality, there is no question that his work here is a good example of the uses to which comparative analysis can be put in dealing with a very complex subject encumbered with a great deal of ideological baggage.

BREWSTER CHAMBERLIN
Institut für Zeitgeschichte,
Munich

JOHN W. COFFEY. *Political Realism in American Thought.* Lewisburg, Penn.: Bucknell University Press. 1977. Pp. 217. \$12.00.

John W. Coffey prefaces this collection of essays on political realism by declaring that he is treating what Jacques Maritain has called "practical philosophy"—an essentially speculative mode of knowledge directed toward action as its object. Political realism to Coffey is not univocal, but means different things to its diverse advocates. Combining a knowledge of both philosophy and history, he discusses the source and essence of the thought of three leading realists—George Kennan, Reinhold Niebuhr, and Hans Morgenthau. Throughout, he applies their realism especially to international affairs.

As one reads each chapter, some mutual themes clearly link the three. All are obsessed with the role of power in politics. All claim to be pragmatists. But beyond these and a few other common bonds, Coffey treats each man's thoughts as a novel theory. Despite the lack of a coherent realist ideology, the end result of this book is a rather clear portrait of what political realism encompasses.

In two chapters devoted to George Kennan, Coffey depicts an ambivalent and at times contradictory figure. Kennan is a humanist. His humanism, combined with his unique personality traits, intrudes upon his political judgment, despite his advocacy of a coldly objective analysis of the political situation. For example, he holds a pronounced animus toward the Soviet Union that slants his impressions of that country. Ironically, in Coffey's opinion, the man who denounces American moralism and legalism injects his own moralism into his judgments.

The liberal theologian Reinhold Niebuhr, according to Coffey, preached Christian realism as an alternative to the secular realism of his day. Starting from the traditional Protestant premise that man is basically sinful, he proceeded to separate morality from politics. Since he felt many contemporaries ignored man's depravity in formulating political remedies, he accused them of being starry-eyed liberals. Yet his own proposals of political pluralism domestically and determined opposition to Soviet Communism externally differed little from their liberal plans. Coffey, therefore, questions the theoretical and practical value of Niebuhr's Christian realism.

To Coffey, Hans Morgenthau is a Hobbesian pessimist who sees all men driven by their passions and appetites, recognizing no intelligible order (such as natural law) in reality. The lust for power constitutes man's ultimate appetite. Since Morgenthau sees all politics as dominated by a search for the proper means to an end, with morality being secondary, might makes right and political theory does not count. Consequently, his own political viewpoints become an exercise in futility.

This is a scholarly critique of political realists.

Coffey bombards the reader with insights which get to the root of the theories of these political realists. At times his writing becomes confusing, as he incorporates so many factors and observations into his work. Yet, on the whole, he presents an admirable comment on the philosophy of political realism.

T. MICHAEL RUDDY
St. Louis University

JOHN T. NOONAN, JR. *Persons and Masks of the Law: Cardozo, Holmes, Jefferson, and Wythe as Makers of the Masks*. New York: Farrar, Straus and Giroux. 1976. Pp. xiii, 206. \$10.00.

Legal history is too important to be left to lawyers and judges, John T. Noonan, Jr. argues, especially when they insist on rendering justice based on poor history. Debunking the myth that the law is an objectifiable entity susceptible to scientific study, Noonan stresses that an emphasis on traditional legal values, such as commitment to procedural rules, impartiality, and doctrinal consistency, masks the humanity of the persons engaged in the legal process. He does not demand an abolition of rules, or legal doctrine, or impartiality, but he does urge the bench and bar to consider the human implications of their actions and abandon a historical method rooted in statute books and case reporters.

Noonan demonstrates the perversity and persistence of masks of the law by analyzing portions of the legal careers of Thomas Jefferson, George Wythe, Oliver Wendell Holmes, Jr., and Benjamin N. Cardozo. While case studies of these scions of the law hardly suffice as proof of the inhumanity of the American legal profession, they do suggest that blind justice is not always equal justice. Thus, Jefferson and Wythe coolly proclaimed the property rights of masters while disregarding slave humanity. Holmes, when confronted with the banana monopoly of the United Fruit Company, retreated to the legal abstractions of sovereignty rather than scrutinizing the company's dubious tactics against its competitors. Even Cardozo, who recognized the consequences of judicial personality in the decision-making process, ignored the humanity of the plaintiff in the famous case of *Palsgraf v. Long Island Railroad Company*; the suffering inflicted in a freak accident by the carrier upon Helen Palsgraf, for Cardozo, was secondary to the formation of a symmetrical rule of tort law. Not only did the railroad win, but Cardozo, who, according to Noonan, "did not see who was before him" (p. 44), ordered the impoverished Mrs. Palsgraf to pay the railroad's court costs.

Law predicated on sound history, Noonan as-

serts, offers an antidote to such myopia. Because legal argument is inherently historical, lawyers and judges would benefit from adoption of a substantially more sophisticated historical methodology. "It is generally accepted," Noonan concludes, "that lawyers and judges are poor historians" (p. 152). Still, he argues, legal historians must share the responsibility; too often they lend credence to misuse of the past by enmeshing themselves in the impersonal abstractions of doctrinal development. Noonan urges legal historians to analyze the "institutions structured by law and served by lawyers" in order to understand "the persons who speak to us through rules and the persons to whom the rules are spoken" (p. 28).

Despite this admonition, *Persons and Masks of the Law* intends to convert Noonan's law school colleagues while reassuring often frustrated historians that they labor not in vain. This gentle pat on the rump may encourage historians, but it does so by belaboring the obvious. Whatever the shortcomings of past scholarship, the present generation of legal historians is already embarked on the kind of scholarly odyssey Noonan recommends. Maxwell Bloomfield, Robert Cover, G. Edward White, Michael Hindus, and A. E. Keir Nash, among others, have grappled successfully with the human consequences of the legal process. Noonan's contribution, therefore, is neither substantive or interpretive; rather, it is to reiterate lucidly for historians the necessity of peering beneath the masks of law, while suggesting to lawyers and judges the value and urgency of a closer alliance with history.

KERMIT L. HALL
Wayne State University

RICHARD P. HALLION. *Legacy of Flight: The Guggenheim Contribution to American Aviation*. Seattle: University of Washington Press. 1977. Pp. xvi, 292. \$15.00.

The name Daniel F. Guggenheim is to American aviation what that of John S. Guggenheim is to the humanities in this country: an important symbol of the private generosity that has stimulated advances in each field. That same generosity through yet another Guggenheim fund made possible the publication of this history of the achievements of the Daniel F. Guggenheim Fund for the Promotion of Aeronautics.

Richard P. Hallion follows the pattern set by previous writers in describing the Guggenheim grants, which committed more than \$2.8 million to aviation projects during the life of the fund between 1926 and 1930; and in arguing that the Guggenheim

intervention came at a critical juncture, impressively strengthening the scientific, technical, and educational underpinnings of aviation. He improves on earlier writers by making more scholarly use of the available sources, by looking beyond the Guggenheim records themselves (as his use of the Hoover papers demonstrates), and by trying to put the work of the fund into a wider context.

The core of Hallion's work is a survey of the principal grants, beginning with a chapter on the aid given to a group of American universities followed by accounts of the fund's cooperative ventures with business and governmental agencies (in creating, for example, a model airline and weather service; in finding solutions to the problems posed by blind flying; and in sponsoring a safe aircraft competition along with promotional tours by Charles A. Lindbergh and Floyd Bennett). Even after the formal termination of the fund, the author reminds us, Guggenheim money helped additional ventures, such as the rocket research of George Goddard.

The result of Hallion's efforts is to deepen our knowledge, a contribution which does not compensate entirely for a propensity toward unnecessary repetition and the inclusion of irrelevant detail. More troubling is the organization of the book. At times, the text seems to be a series of essays; at others, an ongoing narrative. A definite choice as to organization might have made it possible to add to the main text the materials now in two appendixes on the long-term achievements of aeronautical engineering programs aided by the fund.

Many factors must be considered if one is to explain the accelerated progress of American aviation after 1926. Along with the Guggenheim contribution, those factors include the federal legislation beginning in 1925, the work of government agencies, the infusion of tax monies, the confidence inspired by Lindbergh's success, and, soon thereafter, the approach of World War II and the war itself. Hallion touches on all these considerations but only partly measures the Guggenheim contribution against them. Perhaps this book, with its informative essay on the sources and useful bibliography, will encourage another scholar not only to test the prevailing uncritically laudatory opinion of the fund's effectiveness, but to go on from there to evaluate more amply the fund's contribution in the wider context suggested above.

ALFRED F. HURLEY
United States Air Force Academy

MICHAEL J. FRANCIS. *The Limits of Hegemony: United States Relations with Argentina and Chile during World*

War II. (International Studies of the Committee on International Relations.) Notre Dame, Ind.: University of Notre Dame Press. 1977. Pp. xii, 292. \$15.95.

This volume covers relations of the United States with Chile and Argentina from the late 1930s until both Latin American nations declared war on the Axis powers in April 1945. Michael J. Francis eschews philosophies which some use in explaining the meaning of history and adopts a "relatively traditional manner with only a few attempts to fit the findings into any single framework." He concentrates on the "decision-making" policies of the United States, at the same time emphasizing the domestic politics of Chile and Argentina "as the key" to understanding their actions.

The major problem as seen from Washington was the reluctance of both countries to join other American nations in breaking diplomatic relations with the Axis and curbing German influence. Francis examines the role of leading statesmen in Chile and Argentina as well as the intricate political, economic, and social forces which led those nations to oppose United States policy.

He explains events leading to Chile's severance of relations in January 1943 and its belated declaration of war in April 1945, when it was obvious that Germany was defeated and such action was a prerequisite to attendance at the United Nations Conference in San Francisco. Argentina, larger and more influential than Chile, was of greater concern to the United States. The Argentines successfully withstood greater pressure than that exerted against Chile. British policy, different from that of the United States, is clearly analyzed. The story ends with the reversal of both United States and Argentine policy following the Mexico City Conference of February 1945. Scholars will wish that the critical months from Hull's resignation, when Nelson A. Rockefeller began guiding United States policy, until April 1945 had been more thoroughly explained.

There are a few errors. It was April and May of 1940 rather than 1939 when the "blitzkrieg" occurred (p. 26). The "phony war" began in the fall of 1939 rather than in April 1940 (p. 32). Francis states that "Washington's policy had been aimless until December 7, 1941 . . ." (p. 145). He seems unaware of the strong pro-Allied stance of Roosevelt as evidenced by the Destroyer-Bases agreement of 1940, the "shoot on sight" speech, September 1941, and countless other illustrations. Cordell Hull resigned in November 1944, not in December 1945 (p. 238). That date is correctly stated (p. 239), but on the same page Edward R. Stettinius, Jr. is incorrectly called a "bureaucrat with limited expertise on Latin America." He knew little about

Latin America, but Stettinius was no bureaucrat. He had been vice-president of General Motors and chairman of the board of United States Steel before Roosevelt brought him to Washington. The American nations recognized Argentina on April 9, not April 4 (p. 240).

Despite these minor flaws, this is a scholarly volume based on careful research in English- and Spanish-language sources in this country and in Chile and Argentina. It fills important gaps in the narrative of policies emanating from Washington, Santiago, and Buenos Aires during those frustrating years.

EDWARD O. GUERRANT
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Los Angeles

ROGER J. BELL. *Unequal Allies: Australian-American Relations and the Pacific War.* Carlton, Victoria: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1977. Pp. ix, 278. \$18.00.

This is a book that the author would do well to rewrite. In his words, "this book attempts to present a comprehensive analysis of the broadly integrated political, military and economic relations between Australia and the U.S. during the formation, operation and demise of the war-time alliance, and negotiation of the preliminary peace settlement in the Pacific" (p. 3). As it stands, however, no matter how magnificent the topic, this book is inchoate.

How could one get an adequate feel for the American side of the equation by using fewer than a dozen monographs by Americans and then relying almost exclusively on the judgments of Gabriel Kolko? The result is that, even though the author has done considerable research at the National Archives in Washington (but none at the Naval History Division's Operational Archives), his conclusions and interpretations are often faulty. Otherwise, how could one reach, with such certainty, this point: "Indeed, during the war and immediate post-war months, the U.S. consistently sought to undermine Imperial economic and political unity and to restrict British Commonwealth influence in the Pacific" (p. 49).

The author also has not been well served by his editors. The footnotes are often hopelessly confused congeries. The bibliography is dated and has hardly anything listed beyond 1970. Too often the first names of people are omitted in the text. The list of abbreviations and the index are incomplete.

The John Curtin Papers at the National Library of Australia have not been used—even though the Labor Party leader, by index count and emphasis, is one of this history's three most important figures

(along with H. V. Evatt and Franklin D. Roosevelt). In addition to the names of practically all of this country's leading diplomatic, military, and economic scholars of the World War II period, the bibliography lacks such notable Commonwealth historians as W. Macmahon Ball, G. St. J. Barclay, C. Bateson, G. C. Bolton, S. J. Butlin, J. G. Crawford, A. Dalziel, and I. Dowsing, to mention only a few names early in the alphabet.

F. P. KING
Texas Tech University

MAEVA MARCUS. *Truman and the Steel Seizure Case: The Limits of Presidential Power*. (Contemporary American History Series.) New York: Columbia University Press. 1977. Pp. xiv, 390. \$14.95.

In many respects the power of the modern presidency is such that it may no longer be meaningful to classify presidents categorically as "weak" or "strong." In the twentieth century the president virtually has been forced to be a strong executive because the powers of the office are not only those spelled out in Article II of the Constitution but also those established by precedent or authorized by Congress. Precedents established by forceful or politically successful personalities in the office were also available to less gifted successors, and permanently so because of the difficulties that surround the constitutional amending process. Presidents often contend that their power is more potential than real. Harry Truman recognized the ambiguities of Article II which presidents could interpret as either a grant of power or a mere designation of office. Truman was also keenly aware of the immense reinforcement which recognized "emergency" as a meaningful part of his powers. Confronted with emergency, Congress has felt at once the need for action and at the same time its inability to plan the action needed. So historically it turns to the president. Conversely, when the feeling of emergency subsides, opposition to executive power rises correspondingly.

It was against this backdrop of more than twenty years of an activist presidency in which the executive powers had steadily increased that Truman made his fateful decision to seize the steel plants in 1952. This fine book by Maeva Marcus reviews the factors that led to Truman's decision to seize the steel plants, including the difficulties of fighting a limited war in Korea while maintaining an uninterrupted production of steel in the face of a possible strike in the industry. The story is a complex one. The author describes how shocked the country was and how the seizure "triggered a classic constitutional and institutional debate" (p. x). Because Congress did not act, the resolution of

the crisis rested upon the progress of a lawsuit through the federal courts. The Supreme Court's surprise decision in *Youngstown Sheet and Tube Co. v. Sawyer* invalidated Truman's action. This brilliant study assesses the influence of the case on the nature and practice of presidential power as well as on the doctrine of separation of powers.

The author very carefully constructs the narrative of this confrontation, moving from an analysis of the political implications of the seizure through the complexities of its constitutional importance. The book is the best account of the incident to date, and it stands as an excellent example of historical scholarship. This study also examines the value of the *Youngstown* decision in terms of a nation which suffered terribly through the later abuses of power by the Nixon administration when the decision "reaffirmed the basic principle of the rule of law and its corollary that the President, like every other citizen, is subject to the law" (p. 260). One novel note is Marcus' correct assertion that Truman, because of his decision to seize the plants, became an instant hero to rank and file union members. This perhaps helps to explain the origins of the more recent "Trumania" in American life.

The richness of the research, the sophistication of the analysis, and the great editorial skill and care that went into the book will make this a standard work for all students of the Truman era. The author has drawn from all of the major collections of archival sources. Marcus should be congratulated for such a cogent and effective presentation of a most difficult subject.

ARTHUR F. MCCLURE
Central Missouri State University

FRANCIS H. HELLER, editor. *The Korean War: A 25-Year Perspective*. Lawrence: Regents Press of Kansas. 1977. Pp. xxii, 251. \$13.00.

You remember Korea: the war just after the glorious crushing of the Axis powers and just before the ignominious retreat from the Vietnamese peasants. Thinking back to that time (now a quarter-century past), certain obvious images come to mind: Truman and MacArthur, the endless bleak hills, and the seemingly endless peace talks finally terminating in a *status quo ante bellum* settlement.

The present work is a reflective look at that war by some of the major participants (generals, diplomats, bureaucrats), as well as some of the recognized scholars on the Truman period. They were brought together on the twenty-fifth anniversary of the inception of the war by the Harry S. Truman Institute. This book is a compilation of the proceedings. It includes some formal addresses, some

scholarly papers, commentaries on each paper, and transcripts of the open discussions which followed. The most promising section of the book, in the end, disappoints. It is a panel discussion bringing together several key wartime figures: Generals Collins and Ridgway; John Muccio, ambassador to Korea; Ernest A. Gross, a U.N. representative; Averell Harriman, then special assistant to Truman, and other luminaries. Surprisingly, particularly since they have had so many years to reflect, the panelists had nothing new or revelatory to say. The best to be found here is confirmation of what was already known or generally assumed. For example, South Korea was so very aggressive in attacking North Korea that Ambassador Muccio believed at first that the North Korean invasion was just another retaliatory raid. Also, some ranking U.S. officials, notably Harriman and Collins, were unhappy with the performance of General Walton Walker and wanted Ridgway as his replacement.

Two of the scholarly papers included are worthy of note. One is "The Korean War and U.S. Foreign Relations: The Case of NATO" by Lawrence S. Kaplan. As one of the commentators on this paper noted: "historians tend to describe the development of events in Europe and Asia along separate tracks. A major exception is Kaplan's integration of the impact of the Korean War with the building of NATO and the end of American isolationism" (p. 92). Also impressive is John Wiltz's "The Korean War and American Society," which offers a perceptive—if sometimes overstated—review of the domestic impact of the war. Wiltz should now take upon himself the far more complex task of delineating the societal implications of the Vietnam conflict.

This collection suffers from many of the ills common to the genre: unevenness of both style and scholarship and the lack of a unifying narrative theme. Students of the period are obliged to examine these pages to glean out the occasional items of value. Others, with a casual knowledge of the personae or events of the war, will find themselves adrift in a sea of obscure references from the outset.

RICHARD F. HAYNES
Northeast Louisiana University

WILLIAM R. CORSON. *The Armies of Ignorance: The Rise of the American Intelligence Empire*. New York: Dial Press. 1977. Pp. vi, 640. \$12.95.

Not too many years ago serious scholars of U.S. intelligence activities complained of the modest literature on the subject and of the scarcity of original material. A handful of autobiographies of former intelligence officers, an occasional reference

in a presidential memoir, and the usual spate of deliberate leaks or of self-serving after-action reports by intelligence services constituted the bulk of the material available.

The "year of intelligence," 1975, and the Freedom of Information Act (FOIA) revolutionized research on intelligence. A combined total of twenty-two volumes, more than eight thousand pages, was published by the Senate and House Intelligence Committees. These include those volumes related to intelligence produced by the Commission on Organization of the Government for the Conduct of Foreign Policy. Acting under FOIA, scholars and others have also obtained the release of considerable material on the organization, development, and activities of the intelligence agencies. This treasure trove is now being utilized.

In *The Armies of Ignorance* William R. Corson has made considerable use of the newly available material in an ambitious effort to describe the evolution of the American system. Opening with a chapter on the relationship between intelligence operations and the postwar presidents, Corson devotes his second chapter to some of the history of the American intelligence, a subject to which he returns in the tenth and final chapter. The seven intervening chapters describe U.S. intelligence from before Pearl Harbor through the Second World War, with two chapters on the Truman administration, one on Eisenhower's, and one on the period from Kennedy to Carter.

Corson describes in some detail the British effort to assist Donovan in organizing the OSS, which has been described from the British viewpoint in *A Man Called Intrepid* by William Stevenson, and the battle in Washington over the control of intelligence. He goes into considerable detail on Truman's dissolution of the OSS and creation of CIA. He reviews some of the covert operations of the Eisenhower period. His chapter on the future of intelligence already is dated by events but has some interesting viewpoints.

Corson uses the full text of several documents and excerpts from others to support his history of the building of the intelligence community. For example, he includes the sixteen-page Bureau of the Budget "Report on the Intelligence and Security Activities of the Government" of September 20, 1945. The effort may be to demonstrate authority, but the result is excessive length in a book of more than a quarter-million words.

Corson traces the evolution of the U.S. system more comprehensively than previous studies have done, but his account is weak in those areas where no documentation exists, e.g., interpersonal relations. One could argue about his selection of material, his coverage of subjects already well described in other books, and his points of emphasis, but

Armies of Ignorance is a worthwhile addition to the literature of intelligence.

LYMAN B. KIRKPATRICK, JR.
Brown University

RHODRI JEFFREYS-JONES. *American Espionage: From Secret Service to CIA*. New York: The Free Press. 1977. Pp. xi, 276. \$12.95.

All serious research on the largely unrecorded history of the work of intelligence agencies in the United States ought to be welcomed, but scholars will learn little from Rhodri Jeffreys-Jones' new book, *American Espionage: From Secret Service to CIA*.

Unfortunately Jeffreys-Jones, who studied at Cambridge University and is a lecturer at Edinburgh, shows no depth of understanding of the real world of American intelligence in the modern era (post-1947). He has done a lot of dry-bones research in the documentary archives, supplemented by reading of the personal, fallible memoirs in the period down to shortly before World War II. As for the present-day intelligence practices, the book is full of confused and confusing terminology plus semijournalistic speculation on political motivations and bureaucratic purposes.

For example, the title word "espionage," properly referring to clandestine collection of hard-to-get information by secret agents, is often equated with other intelligence activity ranging from research and analysis to covert political action and paramilitary operations. His broad generalizations about the structure and character of intelligence agencies are often totally unconvincing to anyone with a minimum of first-hand familiarity with real intelligence work in the United States. Thus his references to a mysterious entity which he calls "the organization" and describes as a "new group of spies" in the State Department (pp. 194, 199) are total nonsense; he is evidently referring to the Bureau of Intelligence and Research, an open, congressionally funded part of the State Department, with only research analysts and no spies. Further, Jeffreys-Jones thinks that it "is a myth" about American intelligence after World War I "that demobilization was excessive" (p. 145). If you believe that, you will believe anything.

Actually this book mostly presents research on rudimentary American intelligence agencies of the 1898-1940 period. Twelve of fifteen chapters are on the pre-World War II era. They reveal some miscellaneous data on these years of interest to the specialist; these chapters read very much like an American doctoral thesis, something I thought Cambridge University would immunize its students against. In any case, the rest of the book stretches a meager amount of material down to the

present, with a few references to the Office of Strategic Services of World War II and vague allusions to the iniquities of the CIA.

About the CIA Jeffreys-Jones clearly knows little except what he picked up from skimming the Church and Pike congressional committee reports of the 1970s and from various anti-CIA books like those by CIA critics Marchetti and Marks, and CIA defector Philip Agee.

Jeffreys-Jones also should have consulted better-balanced evaluations of the CIA brought out by retired intelligence officers last year—such as those by Harry Rositzke and David Phillips—and my own *Secrets, Spies and Scholars* (1976). He has obviously never heard of them. These recent books or a little judicious interviewing might have given him a better perspective on the last thirty years of American intelligence history, which his book pretends to cover but does not in any way illuminate.

RAY S. CLINE
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HERBERT Y. SCHANDLER. *The Unmaking of a President: Lyndon Johnson and Vietnam*. Princeton: Princeton University Press. 1977. Pp. xx, 419. \$16.50.

This is an interpretive account of why President Johnson changed his Vietnam War policies in March of 1968. It is a serious, well-written, well-documented account by a man who served for several years within the Pentagon and had access to numerous inside documents. It is, as well, a useful scholarly analysis of how Johnson's senior cabinet and military advisers participated in Vietnam decision-making.

Herbert Y. Schandler served on General William Westmoreland's staff in the Office of the Chief of Staff of the Army and had earlier been the principal compiler of the sections of the Pentagon Papers that dealt with the Tet period. In addition to benefiting from unusual access to valuable and still unavailable Pentagon documents, the author's study is informed by a series of thirty-four interviews with key participants. Indeed, his detailed interviews with individuals such as Clark Clifford, Westmoreland, Maxwell Taylor, and others make the book of significant interest.

The author's primary interest is to provide an impartial explanation of what factors caused Johnson to change his Vietnam War policies and refuse to send more troops (that is, higher numbers of troops) to Vietnam in the spring of 1968. His findings are not all that surprising. Clark Clifford's reassessment of the war and his willingness to challenge Johnson's assumptions (and the military's assumptions) about how the war was being

conducted played a key role in Johnson's decision. But Schandler is not satisfied by surface explanations. He wants to know exactly why Clifford changed his views and then how and why Clifford could succeed in changing Johnson's mind. Plainly, Johnson was not the type who liked to be told that his policies were wrong. Indeed, Johnson plainly did not encourage dissent at all.

The Tet offensive in January of 1968 and the forthcoming 1968 elections proved to be key factors, Schandler suggests. He gives them careful scrutiny. Many people insist, and this is Schandler's sentiment, that Tet was actually more of a defeat than a victory for the opposition in Vietnam. But this is plainly not the psychological effect it had here at home. It seemed like an impressive attack on the U.S. forces, and it clearly undermined the official campaign here to convince the public that the war was being won.

The fact that President Johnson was faced with challenges in his own party and a badly divided nation and that the elections of 1968 loomed large in any calculations of what could be done also played a key role in both Clifford's and, eventually, Johnson's reasoning about Vietnam. The mood of the country was impatient and searching for a way to get out of Vietnam.

The interplay between Secretary of Defense Clifford and President Johnson is a striking feature of this book. Clifford began changing his mind on the war soon after he took office in early 1968, just as Tet occurred. He knew well that no president likes to be told that his policies need to be changed. And yet this is precisely what he felt he had to do. Encouraged to do so by a score of top Defense Department civilians and a handful of influential senators, Clifford brought the bad news to Johnson. It just will not work, he told the president. And, predictably, Johnson was upset. Johnson liked to have consensus among his advisers. In Clifford's words: "He was irritated with me The bloom was off our relationship" (p. 245).

Schandler correctly explains that Clifford enjoyed a special status both with the president and in Washington. He had been an effective, loyal, and able adviser to several presidents. His contacts with leading politicians and the leaders in the Congress were extraordinary. His standing in the business community was high. He was persuasive and meticulous in presenting his views. He also was by now a man of independent means, and only reluctantly had he accepted the Defense post to which Johnson had appointed him. He had, in effect, done Johnson a favor. Now Clifford had to risk his friendship and even his position to tell his client that he could not win the war that client had been determined to win. Clifford's strategy to help turn the president around is ably told.

Further, Schandler is an effective analyst of the military position in these and related decisions. The military, he implies, were put in an almost impossible position. They were not given the troop strengths to do the job effectively, and they were asked to achieve more than could be achieved given the circumstances in Vietnam. Schandler concludes that the war effort was generally misdirected. There was, he writes, too little realization of the revolutionary dynamics of the situation, of the popular appeal of the opposition and of the weakness of the half-formed, traditionalist regimes that tried to rule there. "The American failure was caused by the lack of realization that military power alone could not solve what was basically a political problem" (p. 343).

The Schandler volume is an important and helpful account of the final months of the Johnson administration and the turning point in the tragedy of the Vietnam War. It should be read by students of this era and students of military politics and the presidency.

THOMAS E. CRONIN
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ROGER MORRIS. *Uncertain Greatness: Henry Kissinger and American Foreign Policy*. New York: Harper and Row. 1977. Pp. viii, 312. \$10.95.

Henry Kissinger will certainly go down in history as a great statesman, but not necessarily for reasons he would fancy and not necessarily without some black marks against him. As history is rewritten, reputations are reconstructed along the way. In this book Roger Morris begins to put the accomplishments (and failures) of Kissinger into historical perspective. As the author indicates, the book "makes no pretense to be either a comprehensive record of American foreign policy during 1969-1976 or a conventional biography" (p. vii). Rather it is an attempt to "give some analysis of and insight into the man and the main thrust of his diplomacy" (p. viii).

The book traces Kissinger's thought as revealed in his early writings, details his establishment of control over the foreign policy bureaucracy through the National Security Council machinery, and analyzes his use of that control to dominate American foreign policy. Actions and motives concerning detente with Russia and China, negotiations in Vietnam, Africa, and the Middle East, and crises in Chile, Biafra, and Pakistan are examined with insight and skill.

An important contribution of the book is the picture it portrays of the affinities and interactions between the disparate personalities of Kissinger and Richard Nixon. Ultimately, the power and

personality of American foreign policy were reduced to that singular relationship and, in the end, the author contends that foreign policy became hostage to the political fate of those individuals. "It is frightening to think," the author states, "of another man, of any of the establishment figures before or after him, facing Nixon and that government" (p. 299). The real enemy in the eyes of Morris, however, is the foreign policy bureaucracy and the eastern establishment figures regularly called in to run it. Their provincialism, shallowness, intellectual mediocrity, affinity for the past, and bureaucratic sloth were responsible for the shambles of American policy at the beginning of 1969. Thus, Morris admires Kissinger's conception of American foreign policy, his NSC organization which circumvented the bureaucracy and created new and innovative policy alternatives, and his manipulation of the bureaucracy to his own ends.

But Morris is ambivalent, in the end, concerning Kissinger. The characteristics which detracted from Kissinger's achievements—expediency, deceit, lack of deeply held principles, secrecy, lack of faith in democracy—are shown and deplored. Kissinger's pre-eminence in foreign policy, the author states, lay less often in himself than in the mediocrity of the government's foreign policy structure as well as the personal weaknesses of his bureaucratic rivals. And worst of all, to the author, Kissinger squandered the opportunity as secretary of state to restructure and rejuvenate that State Department bureaucracy.

This interesting, plausible, and well-reasoned book gives an intimate and convincing critical insight into the personality, motives, methods, thinking, and weaknesses of Henry Kissinger. It details his lasting achievements as well as the personal qualities and characteristics which marred many of those achievements and possibly prevented others. In the end, however, Morris judges, "In his apparent brilliance and virtuosity he is the standard by which diplomats are to be measured for years to come" (p. 298).

HERBERT Y. SCHANDLER
Congressional Research Service,
Library of Congress

CANADA

G. P. V. AKRIGG and HELEN B. AKRIGG. *British Columbia Chronicle, 1847-1871: Gold and Colonists*. Vancouver, B.C.: Discovery Press. 1977. Pp. xxi, 439. \$17.50.

This book is exactly the kind that the student of British Columbia history does not need. Given the relatively underdeveloped state of historical writ-

ing in British Columbia, historians should be producing well-researched books that offer new insights into specific aspects of the province's history rather than general histories which recycle the tired old arguments of the past.

This volume is the second in what promises, I fear, to become a series. Even if one were to accept the curious year by year approach to history, which results in the description rather than the analysis of events, the book still has major weaknesses. While the manuscript repositories containing British Columbia material in Canada, the United States, and Britain are referred to in the bibliography, the footnotes indicate that the authors prefer to rely on outdated secondary sources. Too many of the publications of the last twenty years are ignored, and graduate theses, which have made so many important contributions, apparently have not been consulted at all. In the light of more recent and reliable work, it is now quite ridiculous, for example, to put one's faith in W. W. Walkem's *Stories of Early British Columbia* as a source of information on Indian-white conflict (pp. 82, 128-29). Evidently the authors are not aware that Walkem's accounts are "stories" in every sense of the word.

While *British Columbia Chronicle* purports to be a catalogue of events, a very definite view of the period is also expressed; and it is as dated as the sources used. British Columbia was a bastion of Britishness. The native Indians formed the majority of the population throughout the period. They numbered approximately 25,000—in spite of a quotation from another historian which informs us that there were "about 8,500 people" in British Columbia in 1869 (p. 379)—yet accounts of the Indians are almost entirely confined to descriptions of attacks on settlers. The authors show no understanding of the Indian cultures or of the acculturative pressures that the Indians faced. The only function of the Americans who came to the area was to make a nuisance of themselves, and even the few hundred Chinese threatened to take over (p. 199). While "respectable persons of the middle class" were busy administering the colonies and making money, the "disreputable persons of the lower class" were brought before courts on charges of drunken assault (pp. 181-82). Valiantly keeping all these tensions under control was the governing factor, James Douglas, who could do no wrong. It is all so reminiscent of the general histories of fifty years ago.

There is much trivia for those who like that sort of thing. We learn how Bruin Bay was named and that the first piece of music composed in British Columbia was "The Vancouver Island Waltz," while, in the section dealing with 1863, the four funerals of "Cariboo" Cameron's wife receive as

much attention as a review of the governorship of James Douglas. With the single exception of some new information on the death of Governor Frederick Seymour in 1869, this book will disappoint the serious student who seeks an enlarged understanding of the colonial phase of British Columbia's history.

ROBIN FISHER
Simon Fraser University

LEO PANITCH, editor. *The Canadian State: Political Economy and Political Power*. Buffalo, N.Y.: University of Toronto Press. 1977. Pp. xi, 475. Cloth \$25.00, paper \$7.50.

The Canadian State is a collection of essays written by young Canadian Marxist scholars. Marxism, editor Leo Panitch admits, came very late to Canadian intellectual life. Unlike their European and American counterparts in the 1930s, even leftist Canadian intellectuals rarely turned to Marxism for a systematic critical theory or for a political faith. The Canadians, therefore, did not pass through that experience of doubt, deception, and rediscovery which has marked the history of radical sensibility in the United States and Western Europe. And yet the scholars writing in *The Canadian State* sound very much like Marxists elsewhere. The Canadian Marxists' freedom from past embarrassments and burdens has not led to a distinctive, original approach in the application of Marxist theory to Canadian society. *The Canadian State* offers many valuable insights and some new information, but it does not accomplish its aim: the construction of a theory that would "systematically address Canada's unevenly developed and dependent economy."

The problem begins with Panitch's introductory "theme essay" on the role and nature of the Canadian state. He carefully sketches the theories of the state developed by Ralph Miliband, James O'Connor, and Nicos Poulantzas and follows this with brief, eclectic comments on Canadian economic history since the mid-nineteenth century. Panitch's essay was written first and then was circulated to the other authors who responded to his arguments. The result is an irritating repetitiousness: Miliband, O'Connor, and Poulantzas are continually mentioned for no apparent reason but with most unsatisfactory results. Marx himself is surprisingly absent; the three exegetes are everywhere, even though none has written specifically or generally on the Canadian state. Indeed, one of the authors, Garth Stevenson, points out that O'Connor's and Miliband's description of the erosion of local powers and the inexorable movement toward centralization in advanced capitalist states

simply does not apply to Canada where the provinces have become increasingly stronger. Stevenson's essay should have cleared the air for a fresh debate, but later essayists ritually genuflect towards Miliband, O'Connor, and Poulantzas before beginning the discussion of their topic. Fortunately, some of these discussions are extremely interesting and important contributions to Canadian political economy.

The best essay is Reg Whitaker's thoughtful study of images of the state in Canada. He sees the events of the past decade as the unraveling of the system which appeared after World War II when welfarism, Keynesian economics, and American capital combined to establish a new role for the Canadian federal state. In the seventies, the Canadian state's historic problem, the lack of "an integral nationalism," has re-emerged with perhaps fatal consequences for that state's future. Certainly Garth Stevenson is pessimistic in the following essay. He declares that decentralization, the most fashionable political nostrum in Canada today, is a fraud, a process which would mainly serve the interests of Canadian and American capitalists. Although Stevenson's tone is often too strident, the essay is provocative and generally well argued. Henry Milner is more optimistic about the consequences of Quebec nationalism, but, like Stevenson, he doubts the "progressive character of the independence movement in Quebec."

The remaining essays deal with diverse topics such as Alberta's development strategy, Canadian class structure, economic policy, health reform, and even "the business of art." Common themes are few, and the essays defy general summary. Some do not reflect recent research (Swartz on health policy), and some dwell too long on the trivial (Olsen on state elites and Loney on citizen participation). Except for David Wolfe's useful study of recent economic policy, all could have been briefer. In summary, then, *The Canadian State* is like the country it studies: the sum of its parts is greater than the whole.

JOHN ENGLISH
University of Waterloo

LATIN AMERICA

NIGEL DAVIES. *The Toltecs, Until the Fall of Tula*. Norman: University of Oklahoma Press. 1977. Pp. xviii, 533. \$14.95.

In his book, *Maya History and Religion* (1970), the late Sir Eric Thompson noted that students of Mesoamerican prehistory largely fail to exploit the full range of data available to them and argued for the conjunctive utilization of both archeological

and literary sources in approaching reconstructions of pre-Cortesian history. *The Toltecs* is the most recent attempt to implement Thompson's suggestions. Like many of its predecessors, it fails to accomplish the desired melding and instead weds achievements of notable merit in one area with serious deficiencies in the other.

Nigel Davies makes clear his intent to produce a conjunctively based history of the Toltec people in a foreword and introduction which manifest a pronounced yet oddly unacknowledged debt to Thompson's essay on the subject. Following this, he takes up in an incisive and illuminating manner the problem of alternative interpretations for the locative and nominative designations, "Tollan," "Toltec," and "Quetzalcoatl." Here as throughout the book, Davies' presentation and assessment of ethnohistoric sources can only be described as thorough and perceptive.

Davies' historical reconstruction begins with the collapse of the imperial urban capital, Teotihuacan, and the probable impact of this on contemporary Mesoamerica. It proceeds through a consideration of possible Toltec origins and affinities; the establishment of a Toltec capital at Tollan Xicocotitlan (Tula, Hidalgo); the extent and nature of a Toltec empire; and the ultimate decline and eclipse of Tollan Xicocotitlan. Throughout, there is an uneasy joining of sophisticated ethnohistorical interpretation with simplistic archeological gleaning. The volume succeeds as a fresh ethnohistoric assessment of the Toltecs, but fails as a conjunctive reconstruction of their history.

Davies' handling of the written sources relevant to his subjects is hard to overpraise. His documentation is thorough, his analyses well reasoned, his interpretations convincing. Of particular value are his appendixes setting forth in graphic form the source materials bearing on the chronology, personages, and events of the Tula downfall as he has reconstructed it. Whether in accord or not, other Mesoamericanists will find these of immense utility.

Probably of greatest significance among the volume's contributions are Davies' reconstructions of the dualistic Nonoalca-Tolteca Chichimeca nature of Tula Toltec origins, and of the relatively limited extent of their empire. He convincingly argues that the latter encompassed little beyond a roughly ten thousand square kilometer portion of the south-central Mesa Central and that possibly even this was co-ruled with Culhuacan. Also of considerable importance are his reassessments of the chronological and interpersonal relationships of Mixcoatl, Topiltzin, and Huemac; and of the ultimate destination of Topiltzin (Quetzalcoatl) following his departure from Tollan Xicocotitlan. Davies places this in the Valley of Mexico rather than to the east.

In each of these instances the author presents a fresh and convincingly argued perspective on the issues concerned.

Davies' mastery of the ethnohistoric sources and adeptness at interpreting them is not matched by a complementary facility with archeological ones. The study of human behavior through its material residue has made impressive strides over the last twenty years, but Davies appears to be far from aware of these. His employment of archeological data can only be characterized as naive; his lack of appreciation for its potential is well exemplified by his own observation (p. 5) that "archaeology is admirably fitted to serve as the handmaiden of history, surely its ultimate purpose." It is, perhaps, peevish to criticize the idiosyncratic spellings of established phase and location designations (e.g., "Tepeuh" for Tepeu, p. 79; "Dzibilchaltun" for Dzibilchaltun, p. 79; "Tichchel" for Tixchel, p. 198); it is considerably more difficult, however, to overlook the ascription of key radiocarbon dates from Balankanche Cave, east of Chichen Itza, to the ceremonial center, Kabah, some 130 kilometers to its southwest (p. 132); or the assignment to the Puuc zone of northern Yucatan of the Rio Pasion site, Seibal (Sayil?) (p. 195). Such carelessness is, perhaps, made understandable by the author's unfortunate attitude regarding archeology, but its ultimate consequence is the failure of his study to achieve its intended goal. In this same vein, what is by far the least satisfying section of the book deals with the Tula Toltec florescence between A.D. 1000 and 1100. A dearth of ethnohistoric material referential to this interval is only partially compensated for by a rather superficial and unconvincing presentation of some archeologically derived surmises. The few strengths of this chapter (six) lie in Davies' analysis of the demographic and political situations as inferred from ethnohistoric sources. Informed utilization of R. Diehl's edited volume, *Studies of Ancient Tollan* (1974), might well have remedied this situation. Coupled to this archeological naiveté is a correspondingly simplistic view of culture dynamics. Periodic, lemming-like migrations and withdrawals are presented as the primary, if not the only, mechanisms of culture change, and much of the "explanatory" portions of the book comprise no more than a cataloguing of these. The chapter (five) covering Tula-Chichen Itza relationships is particularly weakened by this.

In conclusion, *The Toltecs* constitutes a major, creditable contribution to the literature of Mesoamerican ethnohistory. It is far from being the final word on Toltec history, but as one scholar's speculative reconstruction thereof, and a painstaking collation and assessment of relevant literary sources, it will prove an indispensable work to

students of Mesoamerican prehistory for many years to come.

JOSEPH W. BALL
San Diego State University

RICHARD E. W. ADAMS, editor. *The Origins of Maya Civilization*. (School of American Research Advanced Seminar Series.) Albuquerque: University of New Mexico Press. 1977. Pp. xvi, 465. \$20.00.

This book is based on an advanced seminar sponsored and supported by the School of American Research held during October 1974 in Santa Fe, New Mexico. The theme of the seminar, the origins of Maya civilization, is very well expressed in the fifteen papers presented in the book. The introduction, by Richard E. W. Adams and T. Patrick Culbert, provides a common base for the included papers. Gordon R. Willey in the final chapter examines, synthesizes, and interrelates the processes and models presented earlier in the book. At the end Willey presents an "overarching model" that attempts to sum up the conclusions of the seminar.

The remaining thirteen papers are organized into three main sections which bring together many of the elements, in order of importance, that contributed to the rise of Maya civilization. All papers are liberally sprinkled with references from which data were derived to support theories, processes, and models. Every reader of the book will find the master chronology chart (Figure 1.3), prepared by Joseph W. Ball, invaluable in correlating the phases of growth of the sites discussed in the various papers.

Serious students of the ancient Maya will be delighted to find that this work is an excellent companion piece to the earlier publication, *The Classic Maya Collapse*, edited by T. Patrick Culbert. The fact-filled chapters represent intensive scholarly research, and the meticulous fitting of well-established data into propositions and theories produces carefully drawn conclusions. This work is a fine addition to the School of American Research Advanced Seminar Series.

WILLIAM R. BULLARD, JR.
Pembroke State University

GEORGES BAUDOT. *Utopie et histoire au Mexique: Les premiers chroniqueurs de la civilisation mexicaine (1520-1569)*. Toulouse: Privat. Pp. xii, 554.

This book is an indispensable guide to an important body of sources of information on ancient Mexico and holds much interest and value for all students of sixteenth-century Mexico. Scholars

have long been aware that we owe the greater part of our knowledge of the civilization of pre-Cortesian Mexico to a small number of Spanish religious, mostly Franciscans, who in the half-century after the Conquest established what may justly be called the first school of anthropology in the Americas. The monumental Franciscan ethnographic achievement divides into two stages. The first extends from about 1533 to 1558 and is featured by the work of Fray Andrés de Olmos, who "opened up all the roads that led to the heart of Mexico," and three other missionary-scholars: the famous Motolinia (Fray Toribio de Benavente) and two lesser-known figures, Fray Martín de la Coruña and Fray Francisco de Las Navas. The second runs from 1558 to about 1585 and is filled by the prodigious activity of Fray Bernardino de Sahagún, whose *Historia general de las cosas de la Nueva España* was the supreme achievement of the Franciscan ethnographic school.

Fate was unkind to these scholarly workers in the Indian vineyard. Under Philip II an anti-Indian reaction triumphed in the Spanish court. Beginning in 1577, a series of royal decrees ordered the seizure of Sahagún's manuscripts and similar writings and banned "the writing, in any language, of things touching the superstitions and ways of life that the Indians had." As a result many of these works have totally disappeared or have survived in a mutilated or fragmentary state. Fortunately, other writers who still had access to these manuscripts incorporated substantial portions in their own works; an especially important role in salvaging the Franciscan ethnographic heritage was played by the Spanish judge Alonso de Zorita and the Franciscan chroniclers Gerónimo de Mendieta and Juan de Torquemada.

Wisely limiting his attention to Sahagún's precursors, Baudot has given us an inventory of their writings that attempts to establish their sequence, reconstruct the organization of lost or mutilated works, and settle the question of authorship where unknown or disputed. He has managed to bring a large measure of order and clarity into a field of study hitherto marked by disorder and such flights of fancy as Edmundo O'Gorman's effort to prove that Motolinia did not write his *Historia de los indios de la Nueva España*.

I find less successful Baudot's attempt to prove that the Franciscan ethnographic achievement served not only the ends of Indian conversion but a secret grand design whose chief mover was Motolinia: the design of laying the foundations of the millennial kingdom promised by the Book of Revelation. That kingdom was supposedly to be established in New Spain with the aid of the Indians, purged of their paganism, preserved in their pristine virtues, and separated from the corrupting

influence of Spaniards. Baudot's arguments strike me as fragile. In the first place, he unjustifiably assigns a virtual monopoly of the messianic, millenarian vision to the Franciscans. That vision was shared by many members of the reformed mendicant orders. If Motolinia was a millenarian, so was his great adversary, the Dominican Bartolomé de Las Casas, who waged a more consistent struggle to create ideal Indian commonwealths directed by model religious and separated from Spanish influence. Motolinia supported the *encomienda*, an instrument of hispanization; Las Casas strenuously opposed it. In other ways Motolinia showed that he did not favor total separation of Indians and Spaniards. To be sure, after 1560, in an atmosphere of growing Franciscan despair over the decline of their fortunes and the failure of their program for reconciling the interests of Indians and colonists, some Franciscans drew closer to the Lascasian position by advocating total segregation of the Indians from the Spaniards, with the friars given a free hand in governing the natives. But this was almost the exact opposite of Motolinia's program for Spanish-Indian coexistence, with colonists to be supported by Indian tribute and labor.

Baudot speculates that the separatist conspiracy of Martín Cortés (1565–66) may have had "millenarian implications" and that Motolinia and Gerónimo de Mendieta must have lamented its failure (p. 502). Yet in a letter to Philip II of February 28, 1564 (published by France V. Scholes and Eleanor B. Adams in *Cartas del Licenciado Jerónimo Valderrama* . . . [1961]), Martín Cortés supported an increase of Indian tribute burdens and the imposition of tithes on the Indians (the Franciscans vehemently opposed both proposals) and complained of the pride of the friars and their undue influence over the natives. He seems a most unlikely candidate for ruler over that millennial kingdom of which Motolinia is supposed to have dreamed.

BENJAMIN KEEN
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JACQUES LAFAYE. *Quetzalcóatl and Guadalupe: The Formation of Mexican National Consciousness, 1531–1813*. Foreword by OCTAVIO PAZ. Translated by BENJAMIN KEEN. Chicago: University of Chicago Press. 1976. Pp. xxx, 336. \$22.00.

In conception this book possesses a sweep and grandeur seldom encountered in Mexican historiography: it is an attempt to portray the evolution of a Mexican national consciousness, to delineate the making of the Mexican mind. As do most intellectual histories, this one offers insights and persuasions, some of which, depending upon the reader's grasp of Mexican history and histori-

graphy, will be recognized as brilliant or mistaken, or perhaps both. Certainly the author's primary responsibility is to convince the reader that his sources are sufficient and sound and that his interpretation of them is fundamentally right, which is to say that the finished work possesses historicity. In the present case, Jacques Lafaye is going to win some points while losing many. First is the matter of method: in his opening profession of historical faith the author shuns structuralism (which he considers to be abstraction) and quantification (which he believes to be nothing more than an aid) and is not at all willing to settle for chronological narrative. It is through the history of belief that he chooses to inquire into the Mexican collective consciousness, and it is on the historical function of Quetzalcóatl and Guadalupe myths in the development of the Mexican nation that his thesis is said to be founded. The author thus assumes a phenomenological stance, placing stress on what happened in history as opposed to a structuralist analysis with its hidden meanings and unconscious motivations, and free of the burden of quantified fact.

An immediate problem is that over the three colonial centuries there were simply too few writers and cultural commentators and not enough written by them to document in a convincing way the actual historical functioning of the Quetzalcóatl myth in the society at any given time, to say nothing of generations; this carries down to such elementary terms as popular cognizance, acceptance, rejection, and so on. Lafaye uses the obvious historical and literary sources, but chooses to rely more heavily upon an inferential structure of his own creation which gives his thesis on the Quetzalcóatl myth a certain vulnerability: while he provides valuable insights into several aspects of the development of the myth in the seventeenth century, his linkage of it with revolutionary Creole nationalism of the nineteenth is much too conjectural to support the causal relationships that are implied. He assures us that the "enigmatic expression of hybrid myths in creole emblematic language represents the maximum interiorization of the fusion of cultures; indeed, the existence of new myths and their original expression is the birth certificate of a new culture" (p. 301). That may well be true, but it follows more from over-extended generalization than from an analytical comprehension and demonstration of the dynamics of the myth in the society under examination. This is a strangely ahistorical kind of intellectual history.

The sources and literature of Guadalupe cult in Mexico are better known and more plentiful, and Lafaye's treatment of that mythology is superior to that of Quetzalcóatl. With exceptions one finds his

use of inference more disciplined and better supported by the sources. There is, however, an inexplicable void: in a study that is much involved with symbols and oppressed masses and messianic movements of liberation (to which the Apostle Saint Thomas-Quetzalcóatl and Guadalupe-Tonantzin are held to be mythical responses), Lafaye makes no attempt to understand or take into account the social, religious, and emotional climate of Aztec society on the eve of conquest, for which there are abundant sources and an extraordinary secondary literature. Nor does he understand the psychology of conquest; he is persistently indifferent to Cortés' employment of the Virgin in the struggle for Mesoamerican sovereignty and to the dynamics of messianic movements that were afoot and which ultimately permitted Cortés to win. Such was the matrix that yielded the Virgin's domination of syncretism and transculturation, dual processes which, over the span of three centuries, inexorably produced *lo mejicano*. Lafaye's papier-mâché Virgin does not really make the scene until her prodigious appearance at Tepeyac in 1531. Such omissions are significant only because they raise questions about the author's conclusions. One looks in vain for reflections of illuminating and directly relevant studies by José Miranda, Lesley Byrd Simpson, Woodrow Borah, Sherburne F. Cook, Silvio Zavala, Charles Gibson, and François Chevalier. They never made the scene at all.

R. C. PADDEN
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PIERO FERRUA. *Gli anarchici nella rivoluzione messicana: Praxedis G. Guerrero*. (Biblioteca delle Collane "Anteo" e "La Rivolta," number 16.) Ragusa: La Fiaccola. 1976. Pp. 163.

Published by an Italian anarchist group in Ragusa, Sicily, Piero Ferrua's biographical portrait of Praxedis G. Guerrero (1882–1910) is the first of a projected series of monographs devoted to the anarchists' contribution to the Mexican Revolution. Noted scholars like James D. Cockcroft have treated Guerrero as a secondary figure in the Precursor Movement that spurred the revolution against Porfirio Díaz. Even anarchists have tended to regard Guerrero primarily as the able lieutenant of Ricardo Flores Magón, the famed leader of the *Partido Liberal Mexicano*. It is Ferrua's belief, however, that Guerrero should be brought out from under the shadow of Magón and recognized as an important figure in his own right. The author's favorable reassessment rests mainly on two assumptions: that Guerrero's insurrectionary undertakings helped generate the revolutionary momen-

tum that ultimately toppled Díaz in 1911; and that Guerrero was a precursor of the modern Latin American guerrilla fighters, particularly the "Self-Defense Groups" of rural guerrillas who have been active within recent years in the state of Guerrero, Mexico, and who allegedly adopted the revolutionary tactics of the martyred anarchist.

A son of rich landowners, Guerrero rebelled against his Catholic education while still an adolescent and embraced anarchism at age twenty-one after reading the Magón brothers' newspaper *Regeneración*. Abandoning university education, military career, and family wealth, Guerrero emigrated to the United States in 1904 and became active among Mexican anarchist groups in Texas, Arizona, and California. Although not one of the founders of the PLM, Guerrero was admitted to the PLM's *Junta Organizadora* in 1906 and a year later was appointed its special delegate and secretary. From this time until his premature death in 1910, the self-taught Guerrero also distinguished himself as one of the movement's finest journalists. The development of his promising talents as a writer, however, were periodically interrupted by his conspiratorial and insurrectionary enterprises. Ferrua does not include Malatesta among Guerrero's ideological mentors, claiming that the Mexican was influenced mainly by Tolstoy, Ferrer, Bakunin, Kropotkin, and Reclus. Yet the revolutionary doctrines espoused by Guerrero are very reminiscent of the great Italian anarchist. Like Malatesta, Guerrero emphasized that the anarchists should always maintain a revolutionary presence among the masses through propaganda and direct action. Embodying the desired fusion of revolutionary thought and action in his own person, Guerrero participated directly in almost all the insurrectionary undertakings launched by the PLM between 1908 and 1911. It was during one of these guerrilla actions—the capture of Janos in Chihuahua on December 30, 1910—that Guerrero was killed at the head of his men. A judicious evaluation of Guerrero's death is provided by Diego Abad de Santillan in the preface to Ferrua's volume. Echoing Max Nettlau's opinion on the futile death of Gustav Landauer, the well-known Spanish anarchist reluctantly admits that intellectuals like Guerrero were infinitely more valuable to the movement alive than as dead martyrs. For in de Santillan's judgment—seconded by Ferrua—the loss of Guerrero was a catastrophe for Mexican anarchism.

Despite the author's obvious libertarian sympathies, this study does not fall into the category of movement hagiography. Diligently researched (a few important sources are still unavailable) and clearly written, Ferrua's book constitutes an important addition to the literature on Mexican an-

archism. He has succeeded admirably in raising Guerrero from undeserved obscurity and has further elucidated the important role played by anarchists in the Mexican Revolution. On the other hand, few readers will accept Ferrua's designation of Guerrero as a great precursor of contemporary guerrillas in rural Latin America. Neither an original theorist of revolution nor an exceptional tactician and combatant, Guerrero does not merit any special niche in the guerrilla fighters' hall of fame. Be that as it may, any further study by Piero Ferrua on Mexican anarchism, especially one devoted to Ricardo Flores Magón, would be most welcome.

NUNZIO PERNICONE
University of Illinois,
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FRANKLIN W. KNIGHT. *The Caribbean: The Genesis of a Fragmented Nationalism*. (Latin American Histories.) New York: Oxford University Press. 1978. Pp. xiii, 251. Cloth \$12.50, paper \$4.00.

Histories of the Caribbean region have ranged, over the centuries, from the history of a single island to that of a single European nation's holdings to Caribbean history as an aspect of European power politics. Finally, there were histories of the region as a whole, the best of which, nevertheless, still retained an essentially European point of view. Franklin W. Knight's new Caribbean history is a valiant effort to write about the Caribbean area in a new and long-needed way. Knight has written from an international Caribbean and New World perspective rather than from the more traditional European and imperial point of view. He prefers to emphasize cultural commonalities rather than political chronology, and socioeconomic considerations rather than political ones.

Caribbean political geography before 1492 sets the scene for the arrival of the Spanish and their establishment of settlements and colonies. Later, subjects of other European nations settled in the Guianas and on West Indian islands. Knight evaluates the patterns of colonization to show two kinds of colonies, settler and exploitation, and the meaningful societal differences between them. His discussion of the relationships between European imperialism and slavery in West Indian exploitation colonies serves as a prelude to a thorough examination of the social structure of the plantation society that evolved in the sugar colonies.

Knight delineates an era of disintegration and reconstruction (1793–1886), followed by one of Caribbean nation building (1804–1970). The collapse of the sugar market brought an end to

slavery, and societal changes. At the same time Haiti became independent and as such served as an example for other Caribbean peoples, in spite of continued European colonialism and the increasing role of the United States in the area. Finally, the contemporary scene, beginning with Castro's Cuban Revolution in 1959, is examined from the point of view of Caribbean nationalism, which is seen as separate and distinct from all ideologies brought to the region.

This Caribbean history from a Caribbean point of view has succeeded—perhaps too well. By downplaying European aspects of the Caribbean experience Knight has neglected the geopolitical aspects of Caribbean history. Writing Caribbean history as it is understood by Caribbean peoples has led to an essentially narrow and parochial history that views the rest of the world through a Caribbean filter. The book, written from a viewpoint hostile to European imperialism, and lacking any meaningful chronological framework, contains many unusual interpretations of non-Caribbean events. The book's structure and the author's style make this a less than satisfactory first book on Caribbean history. But Knight's points of view—long needed in print—mean that the book has great value and will be thought-provoking for those already familiar with the history of the Caribbean region.

BRUCE B. SOLNICK
State University of New York,
Albany

JILL SHEPPARD. *The "Redlegs" of Barbados: Their Origins and History*. Foreword by PHILIP SHERLOCK. (The Caribbean—Historical and Cultural Perspectives.) Millwood, N.Y.: KTO Press. 1977. Pp. xiv, 147. \$12.00.

Much of the literature on the Caribbean concerns stratified, segmented, pluralistic, and insular societies in the process of painful evolution. Common to the history of all of them is the theme of a European colonizer locked in cultural conflict with the nonwhite colonized. Jill Sheppard's monograph introduces another variant in this culturally complex region: the Barbadian poor white. Although European, this tragic figure, unable to assume the role of paternalistic exploiter in this once profitable British sugar colony, became a victim.

Sheppard traces the history of the poor whites from their seventeenth-century origins down to the present. English, Irish, and Scots were enticed or forced to Barbados to work land that soon became a valuable source of sugar. Although not enslaved, these indentured servants found working conditions poor and chances for advancement slim as

land was engrossed by the more powerful sugar producers. As elsewhere in the Caribbean, the yeoman farmer was replaced by those who were able to assemble a large labor force along with the capital and land necessary for commercial sugar production. African slaves, found to be more plentiful and less expensive than white workers, increased the pressure on the servant class by reducing the opportunities for employment on the estates.

The downward spiral of these "poor buckras" continued as the end of slavery in the 1830s brought additional setbacks. Competition for jobs with the former slaves as well as loss of land formerly leased from the estates in return for militia service further reduced the economic level of the poor whites. Stubborn refusal to accept employment as agricultural laborers and a decreasing physical capacity to perform useful work left them isolated from the rest of society. Scorned by the planters as undeserving poor and ridiculed or pitied by the former slaves, the "redlegs" withdrew into a stubborn subculture which failed to respond to public relief measures, education, or emigration programs.

By the early twentieth century, however, a combination of factors seems to have halted the downward drift of this "joyless company of pariahs." The reform of a segregated school system, a gradual increase in emigration, improved health care, and expanding opportunities for commercial employment improved their condition. At the present time, the "redlegs" have entered the mainstream of Barbadian life and will soon, Sheppard predicts, "become nothing more than a curious if not almost forgotten historical phenomenon" (p. 120).

Sheppard, in her industrious study, has compiled an impressive amount of data on the poor whites of Barbados using sources on the island, in London, and in the United States. Her synthesis, however, suffers from the inclusion of material which does not always relate to a major analytical question or stated theme. Of additional concern is her failure to distinguish adequately between the arrogant, rum-besotted, hookworm-infested, and thoroughly alienated "redshank" and those whites in the middle sectors of society. Some reworking of the sections on population trends also needs to be done so that the tabular information supports the generalizations made in the text.

Jill Sheppard has, nevertheless, written a valuable contribution to the literature. We need more specialized studies of this kind before the social history of the British Caribbean can be seriously attempted.

BRUCE M. TAYLOR
University of Dayton

KENNETH EVAN SHARPE. *Peasant Politics: Struggle in a Dominican Village*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1977. Pp. xix, 263. \$15.00.

To develop this case-study of rural collective action, Kenneth Evan Sharpe lived among the farmers, or campesinos, of a tiny foothill village in a province in the Cibao, the north central heartland of the Dominican Republic, for eighteen months from late 1970. Other than details of its one-crop economy the author tells us little about the village which he calls Jaida Arriba (not its true name). He treats it as a typical, isolated rural settlement of about two hundred thirty-five families. There is ample description of family life under the bitter conditions of illiteracy, high birth rate, hunger, and poor medical care that have plagued rural Dominican society. The case-study is developed around the economic and social forces at work on these unsophisticated villagers, nearly all of them poverty-stricken.

After a few leaders became convinced that joint action could improve their grim economic condition, the remaining growers, in an unprecedented step, openly began to form a cooperative organization to produce and market their sole money-crop of coffee beans. Once they had reluctantly accepted the idea of collective action they went on to break the long-standing control of the middlemen over prices and conditions of sale. Even though the resulting higher prices could not help those who were small landholders or who worked as day laborers, the gain was a major one in the economic life of the village. In a well-written narrative the author analyzes the step forward in terms of a possibly significant, ideologically inspired "peasant struggle" or "peasant movement."

In seeking to explain the acceptance of the ideology leading to the formation of the collective, Sharpe constructs a research model formulated on theories of coercion/conflict, functionalism/integration, and other concepts used by social scientists in their studies of peasant movements. He applies the theories to the villagers in their roles as growers, middlemen, leaders, or followers in developing his premise that the "collective ideology" in Jaida Arriba became a reality only after 1967 when local clerics for the first time had adopted an active role in the life of the village and after a visit or two by government field agronomists working with the clerics. The author concludes that a model using a combination of coercion and integration theories rather than either exclusively would provide an understanding of the events following these visits.

The author's research, however, essentially ig-

nores important background changes that had been taking place in the D.R. outside Jaida Arriba. Had the model recognized the Church's already active social role in the developing nations of the world, particularly in Latin America and especially in the agricultural sectors of the D.R., and the increasing concern of post-Trujillo governments over rural poverty, a slightly different perspective of the so-called struggle at Jaida Arriba would have emerged. This "peasant movement" was, more accurately, only a village consensus with little impact on the national picture. Central government had been under Church attack for several years prior to 1967 because of ineffective efforts to improve the lot of the campesinos. By 1966 the new Balaguer government was heavily committed to "integrate campesinos into the national life." The visits of the priests and agronomists—and therefore the establishment of the co-operative—were the result of an accelerating program to help campesinos in *all* of the rural areas, not just in the Cibao where other cooperatives were already functioning. The coffee cooperative brought little or no benefit to the majority of the villagers; it had a comparable effect on events elsewhere.

I feel more comfortable with simple historic facts than with ideological theories to explain campesino attitudes. Campesinos had been exploited for generations. They invariably resisted "progress," because change had usually left them sadder or poorer. The villagers of Jaida Arriba accepted collective action when the government offered help, when the Church encouraged it, and when they saw that no Trujillo would break their heads if they accepted. I hasten to observe, however, that despite a bibliography virtually free of Dominican entries, this work will provide modest proof for many readers that challenge and change can occur in Latin America without traditional resort to violence, provided that the political environment is progressive. This is no small note of reassurance.

MARLIN D. CLAUSNER
Temple University

HARRISON SABIN HOWARD. *Rómulo Gallegos y la revolución burguesa de Venezuela*. (Colección estudios.) Caracas: Monte Avila Editores. 1976. Pp. 358.

The purpose of this work is to analyze and understand the roots of Venezuela's most important twentieth-century revolution (October 1945) and the ensuing three-year rule of its intellectual and political authors—Rómulo Betancourt and Rómulo Gallegos. This is a political history of the

origins, rise, triumph, and defeat of Venezuela's middle class.

The interpretive model is admittedly Marxist. The author repeats the familiar thesis set forth in Lenin's *Imperialism* (1917), Paul Baran's *Political Economy of Growth* (1957), and André Gunder Frank's *Capitalism and Underdevelopment in Latin America* (1967). The argument is that bourgeois revolutions are ineffective in resolving fundamental national problems in Third World countries inasmuch as the unaltered satellite relationship to the metropolis prevents genuine independence and blocks meaningful social change. Hence the "development of underdevelopment" continues. Not surprisingly, the author concludes that a socialist government would have been much more beneficial to Venezuelans than the Betancourt and Gallegos regimes. The reader is asked to decide for himself whether such an interpretation is artificial or irrelevant.

Harrison S. Howard has done original research on Venezuelan political history. He has ferreted out the intellectual roots of the dictatorial tradition, which he finds in the primitive society and the backward economy. He has skillfully traced the rise of middle-class political intellectuals through analysis of the novels of Gallegos and the political tracts of Betancourt.

There are shortcomings, however, in the author's surveys of the various administrations that have governed Venezuela in the first half of the twentieth century. His analysis of the dictatorship of Juan Vicente Gómez (1908–35) is not thorough, though it is brilliant in places. His discussions of the transitional governments of Eleazar López Contreras (1936–41) and Isaias Medina Angarita (1941–45) are perfunctory and superficial.

The core of this work is found in the last two chapters which analyze and interpret the *Acción Democrática* (AD) party *trienio* (October 1945–November 1948). Howard believes that Presidents Betancourt and Gallegos failed because their policies were bourgeois and evolutionary rather than socialist and revolutionary. While AD is credited with meaningful reforms in health, housing, and education, it is faulted for its attempts to buy off the military, for increasing Venezuela's dependence upon the international petroleum industry, for delaying agrarian reform, and for its gradualist approach to nationalized industry. Its labor policy is criticized for being sectarian and discriminatory, especially with respect to the Communist unions.

While the prevailing view of historians is that AD erred by attempting to change too much too soon, Howard believes its timidity, excessive caution, and gradualism ruined the chances for meaningful reform. He thinks it failed because it could

not come to grips with the fundamental problems of agrarian reform and national industrial development. Hence it was unable to alter Venezuela's satellite relationship to the United States metropolis.

Despite the author's pronounced Marxist bias, this is an important, worthwhile book. It is a meaningful addition to existing interpretations of modern Venezuelan history.

EDWIN LIEUWEN
University of New Mexico

J. R. FISHER. *Silver Mines and Silver Miners in Colonial Peru, 1776-1824*. (Centre for Latin-American Studies, Monograph Series, number 7.) Liverpool: University of Liverpool. 1977. Pp. 150. £2.75.

From the mid-sixteenth century until the middle of the nineteenth century, Peru and Mexico supplied the overwhelming majority of the world's principal commercial currency—silver. The silver-mining industries of both colonies experienced significant revivals in the last half of the eighteenth century when combined production trebled that of the banner years of the late sixteenth century. While Mexico's spectacular boom has been well studied by historians, Peru's lesser success has been largely ignored. J. R. Fisher partially fills this gap in our knowledge with this short and thoroughly documented monograph.

The study focuses upon the attempts of the Spanish Crown to implement policies favorable to the economic development of the silver-mining sector. After a brief survey of silver mining prior to 1776, the author discusses the *visitas* of the 1770s and 1780s and Bourbon efforts to establish more control over the industry. Using government correspondence from the A.G.I. and the Archivo Nacional in Lima, Fisher details the problems of instituting a Mining Tribunal and a new mining code. The infighting associated with the tribunal's endeavors to form silver exchange banks clearly demonstrates the traditional strength of Lima's merchant community. A subsequent chapter sheds new light on the ill-fated Nordenflicht mining mission which intended to introduce an inapplicable technology into the refining sector. An analysis of the most important refining input, mercury, reveals Spain's favoritism toward Mexico in matters of price and distribution. All of the Crown's efforts, save perhaps certain aspects of the mercury policy, were in vain, and the author documents these failures admirably. Fisher's monograph nicely complements his earlier work on colonial government and society, and is therefore of interest to the student of Bourbon politics.

From the standpoint of economic history, the

study falls short of the analysis demanded by the title. Less than twenty pages of text are devoted to the crucial topics of labor, capital, and production. Thus, while the failures of government programs are painstakingly explored, the noteworthy doubling of silver output between 1777 and 1792 receives slight explanation. Similarly, there is disappointingly little about changes in the laboring sector during the important fifty-year period leading to independence. In regard to capital, we do learn that the merchants of late colonial Peru were less inclined than their Mexican counterparts to invest in the mining industry, and that such reluctance hindered an even greater growth in production.

As for the organization of the monograph, the critical connection between mercury supply and silver output would have been less obscure if these two closely related chapters had appeared consecutively. And Fisher notes only in passing the key issue of chronic and widespread contraband. The fact that the author presents sufficient raw data to calculate that contraband comprised a third to a half of total silver production, but does not bring this out in the text, indicates a lack of reflection on this central problem. Despite the obvious brevity of treatment, Fisher's new data on production, the scattered information on labor, and the discussion of some phases of capital formation, make this monograph a modest contribution to the economic history of colonial Peru.

HARRY E. CROSS
Stanford University

GUILLERMO LORA. *A History of the Bolivian Labour Movement, 1848-1971*. Edited and abridged by LAURENCE WHITEHEAD. Translated by CHRISTINE WHITEHEAD. (Cambridge Latin American Studies, number 27.) New York: Cambridge University Press. 1977. Pp. x, 408. \$24.95.

This book explains the development of the Bolivian working class from the guilds and mutualist societies of the nineteenth century to the class-conscious, combative, and revolutionary force of the twentieth century. The author traces the historical process which led through the 1952 National Revolution and the nationalization of the mines to present-day military rule and the repression of the miners. It is argued that "whoever controls Bolivia's mines becomes master of the country" and thus, "proposing control of the mines by the working class implied making the official government unworkable and relegating it to a totally useless role" (p. 366). Unfortunately, one could also put forward the proposition that whoever controls Bolivia's military becomes mas-

ter of the country, as did the United States government when it began rebuilding the institution shattered by a people in arms during the 1952 revolution. The result, treated within this work, was the resurgence of militarism and the bloody repayment of the miners for their historical role in the military defeat of April 1952.

This volume is both a translation and an abridgment of a partisan history written by Guillermo Lora, secretary general of the Trotskyist *Partido Obrero Revolucionario*. The first half of the book has been in print for a decade as a three-volume *Historia del movimiento obrero Boliviano* in the *Enciclopedia Boliviana* series. It is therefore of somewhat less utility to specialists who will prefer the original. The second half of the work is an important contribution to those scholars who lack access to the collections of party militants or the archives of the North American intelligence services. Because of the abridgment (about two-thirds of the Spanish original has been deleted), which removed the bulk of Lora's political argumentation, this English version has a tendency to read at times like an encyclopedia of the Bolivian labor movement.

Christine Whitehead, an Oxford graduate with field experience and a thesis on Bolivian history, has ably translated the book, and the editor, Laurence Whitehead, is the most perceptive of foreign scholars writing on Bolivia. Unfortunately, he has limited his editorial commentary, thereby minimizing the possible contribution of an always exciting analyst. A brief and incomplete bibliography is appended, and the photos interspersed through much of the original work have been excluded, thus leaving the reader's imagination limited to written descriptions of the men and women who comprised the various parties and movements in Bolivian labor history.

The original context of this history involved the attempted politicization of its audience into a Trotskyist perspective, and much of the material will be familiar to those readers acquainted with Lora's series of *folletos* published irregularly as *Ediciones "Masas"* over the last thirty years. Lora's intellectual life cannot be divorced from his politics and as a scholar he has been harassed and pursued by the secret police for years. A good deal of the present volume has been spirited about from Bolivia to Chile and eventually to England. The decision to reprint the original study together with a nearly ephemeral version of the material now presented in the second half of this book is thus a service to Bolivian history.

JAMES V. KOHL
Gulf Breeze, Florida

HENRY W. KIRSCH. *Industrial Development in a Traditional Society: The Conflict of Entrepreneurship and Mod-*

ernization in Chile. (Center for Latin American Studies. Latin American Monographs, Second Series, number 21.) Gainesville: University Presses of Florida. 1977. Pp. xiv, 210. \$10.00.

This book is a significant contribution to Chilean economic history. It focuses on industrialization between the War of the Pacific and the Great Depression, a period scholars have neglected. Based on impressive research in the archives of the Santiago stock exchange as well as in company, trade-association, and government publications, the book challenges several long-accepted interpretations about Chilean industry during this period.

Taking issue with well-known economic historians like Claudio Véliz, Henry W. Kirsch presents and defends a related series of propositions. First, Chilean industry began to flourish during the War of the Pacific and it expanded rapidly thereafter. Second, powerful members of the agricultural and merchant oligarchy, perceiving an opportunity for significant profit, supported industrialization and often became industrial entrepreneurs. Here Kirsch directly challenges the common thesis that industrialists formed a separate interest group that was in conflict with the oligarchy. He demonstrates that merchants, for example, turned to industry by importing raw materials and then by producing and distributing the finished items. A related point is that although only two percent of Chile's population was European-born, about half the industrialists were immigrants. Nonetheless, their interests did not clash with the dominant economic elite; if successful, they were indeed able to join that group.

The analysis of the industrialists' role in the 1891 civil war is particularly interesting. Here Kirsch challenges various Marxist historians and demonstrates that the entrepreneurs opposed Balmaceda. Moreover, far from suffering under the parliamentary governments after 1891, industry actually flourished. By 1918, the monopolistic and oligopolistic patterns familiar in later periods of Chilean industrialization were already well established. Industrialists consolidated their position prior to the depression partly because they were able to enlist the support of the state, particularly in the area of favorable tariff legislation. Although industry became a major sector of the Chilean economy, it did not form a "growth pole" for economic development; entrepreneurs were primarily concerned not with heavy industry but with short-run profit maximization.

Kirsch generally employs his data persuasively; he uses statistics with particular effectiveness. Some of his arguments, however, are overstated. Immigrant entrepreneurs, for example, were by no

means as welcome among the established elite as Kirsch suggests. This was particularly true of Syrian and Lebanese entrepreneurs, a key group in the textile industry, and of Jews. But these rough points are few, and the book's fresh insights are numerous.

CARL SOLBERG
University of Washington

RONALD C. NEWTON. *German Buenos Aires, 1900-1933: Social Change and Cultural Crisis*. (Texas Pan American Series.) Austin: University of Texas Press. 1977. Pp. xvii, 225. \$15.00.

The role and contribution of foreign communities in Latin America have long attracted comments and speculations from travelers, journalists, and politicians. Yet surprisingly little serious scholarly research has focused on these questions, despite the apparent richness of sources and opportunities for provocative hypotheses and comparisons. A spurt of assimilation and acculturation studies emerged in the São Paulo group of *Sociología* in the 1950s and early 1960s, at the same time that Gino Germani, Sergio Bagú, José Luis Romero, and others projected a major investigation into immigration history in the Río de la Plata. For a variety of reasons, including uncongenial political climates, these efforts yielded relatively few monographs and have largely collapsed. And only a few North Americans, such as Carl Solberg, C. Harvey Gardner, and George F. W. Young, have recently exploited this challenging field.

This relative absence of readily available immigration literature makes Ronald C. Newton's first volume in a projected two-volume study of Germans in Buenos Aires particularly welcome and useful. He has deliberately chosen the narrow focus of a single nationality in a single city over a short time span in order to exhaust the sources and to understand better the complex interactions within the immigrant community and beyond that the relationships maintained with the host society.

Certain characteristics of this German community both facilitate and obstruct Newton's task. Germans as artisans, professionals, and merchants played a far more important role than their numbers (increasing from 4,000 in the 1880s to 45,000 by the 1930s) suggest. But measurement and evaluation of their impact has proven difficult. Germans maintained a profound conviction of their superiority over the host culture that contributed to aloofness and isolation. At the same time, Germans exercised considerable influence over the areas of Argentine technology, business, military affairs, and education. Dramatic changes in German society and politics between the eras of Kaiser

Wilhelm and Adolf Hitler, meanwhile, not only resounded within the immigrant community but also affected strongly its relations with the Argentine hosts. The presence of a sizeable group of German-speaking Jews within the German community further divided Germans and has complicated the collection of data and its analysis.

Newton meets most of these challenges and problems successfully. He admits a lack of access to Jewish sources, but nevertheless traces with great detail and knowledgeability the rising tensions within the complex German society, split not only by ethnic and religious distinctions but also by class and economic differences. This portrayal of struggle and conflict, based on exhaustive use of local German newspapers and journals, constitutes the book's greatest strength. Newton also develops an interesting analysis that suggests how Germans became increasingly subject to assimilation pressures after World War I. His final chapters, in obvious prelude to his next study that will deal with the 1933-45 period, illustrate the opportunistic acceptance of Nazism by the community's economic elite.

Some will doubtless criticize Newton for focusing merely on Buenos Aires rather than on Argentina and consequently ignoring rural immigrants, while others may feel that he has not adequately defined or understood the Jewish element. But *German Buenos Aires* serves to correct numerous superficial assumptions about the role of German immigrants in Argentina, and this analysis of a community's internal dynamics makes for a major contribution to immigration history.

JAMES R. SCOBIE
University of California,
San Diego

JOHN D. WIRTH. *Minas Gerais in the Brazilian Federation, 1889-1937*. Stanford, Calif.: Stanford University Press. 1977. Pp. xx, 322. \$17.50.

If Portuguese America had followed the example of the Spanish empire, Minas Gerais would be an independent nation today, rather than one of over twenty states in modern Brazil. Three historians—Robert Levine, Joseph Love, and John Wirth—have collaborated on a project to write the comparative history of many Brazilian states for the period from 1889 to 1937; Wirth's is the first volume to appear.

Minas Gerais makes it clear that the Brazilian federal system, rather than being the artificial creation of constitution writers enamored of the United States, was responsive to the felt needs for autonomy among political leaders at the state level. These politicians also faced the problem of

combating other *mineiros* (residents of Minas Gerais) whose loyalty was to their even more immediate locality. How state leaders played up to these local interests without succumbing to them and how they acted to safeguard state interests at the national level form the basic problems addressed in this book. Unfortunately, Wirth does not focus on them with full success. Besides telling us more than we want to know about the individuals—it is certainly a book for the specialist—he never states these issues clearly.

The major achievement lies in the use the authors have made of the computer to analyze members of the political elites of the three states in terms of their family ties, education, occupation, political careers, links with each other within each state and across state boundaries, foreign ties, and public stance on a number of key issues. Wirth has also presented a detailed discussion of the state budget, state police, and participation of *mineiros* in regional and national congresses.

From all this, however, the author comes to rather modest conclusions even when the data is compared to that emerging from the companion volumes. Although he maintains, correctly, that “the challenge of comparative history” (p. 30) is to explain the differences, the volume is more descriptive than explanatory. The explanations that he does proffer tend to beg the question: If one asks why Minas Gerais was different from the other states, he is satisfied to say that the state political machine was more disciplined; if one asks why these men were more disciplined, he has no answer. Frequently his explanations appear to be the merely accidental presence of individuals with valuable political skills.

Wirth is sometimes contradictory about regionalism. Whereas at one point he says that “for *mineiros* . . . state and region . . . coincided” (p. 230), at others he is at pains to stress that “disarticulated growth” (p. 5) sprang from the fact that the state’s boundaries did not define a natural region and that local leaders focused their attention on other states. Unity of the state “was the ideal of a political elite” (p. 26) but apparently no one else’s. Like many students of elites, Wirth ignores how most people may have felt about the issues, fully identifying himself with the relatively few actors he studies, and railing, for instance, against the fact that some *mineiros* “were all too well integrated” (p. 202) into other state networks. In the end Wirth admits he himself is not really sure what integration means.

He takes issue with the general belief that landowners—ranchers and planters—controlled Minas politics. He often repeats that “*fazendeiros* did not run the state” (p. 226). Yet he tells us that as many as thirty-eight percent of state bureau-

crats belonged to major landed families; that “sustained opposition by landed interests” (p. 120) blocked passage of a land tax for twenty years; and that “typically” the elite politician “kept in close touch with rural relatives on whose *fazendas* he would spend vacations if he did not have rural properties of his own” (p. 157). His discussion of clan and family networks is ambiguous, contradictory, and inconclusive. Perhaps that is all the data will allow, but it is disappointing for a work aimed at studying the social bases of politics and represents a missed opportunity to speak to the important issues of the relationship between the state and society.

Despite all these objections, I should repeat that the book is an important one for the Brazilianist, detailing the political history of this state for the first time and gathering a wealth of data that can flesh out the hitherto merely impressionistic notions we have had about politics in this period.

RICHARD GRAHAM
University of Texas,
Austin

ANNA MARIA MARTINEZ CORRÊA. *A Rebelião de 1924 em São Paulo*. Preface by E. OLIVEIRA FRANÇA. (Coleção Estudos Brasileiros, number 2.) São Paulo: HUCITEC. 1976. Pp. xx, 201.

From the outset things went very wrong. The 1924 revolutionaries of São Paulo had planned a lightning raid on the city’s armories, to be followed immediately by an advance up the Paraíba Valley to attack Rio de Janeiro and force the president to resign. But rather than several hours, four days passed before São Paulo fell, by which time the army had moved into strong positions from which it bombarded the city daily. The rebels were trapped in a city which did not want them and which held no strategic military value. They established an emergency government which ineffectually ran the city of nearly a million people. After just three weeks the revolutionaries abandoned São Paulo and fled into primitive Mato Grosso, the only way not blocked by government troops.

This important new study describes in detail the revolt of São Paulo, an early episode in what came to be known as the *tenente* movement, a series of armed uprisings by cadets and junior officers protesting government corruption and fraud. Upon leaving São Paulo, the *tenentes* marched through the interior for nineteen months, spent several more years in exile, and finally returned in 1930 with Getúlio Vargas’ revolution. The prominence they achieved under Vargas has induced historians to romanticize their exploits during the 1920s. Anna M. Martinez Corrêa’s contribution is to

document the mismanagement and failure which largely characterized this revolt, basing her account on voluminous police records, newspapers, and memoirs. First, the rebels had no contingency plans and never fully recovered from their early setback. Worse, they published such inane reform programs (e.g., separation of Church and state, honest elections, respect for the 1891 constitution) that virtually no organized civilian support surfaced. Rather than improvise a truly revolutionary alliance with the city's industrial workers, the rebels gave administrative functions to the local bourgeoisie. The latter managed to minimize artillery damage to the city and to prevent labor unrest during the crisis. Thus when the revolt was over, nothing could be said to have changed. Later, of course, the *tenentes* became far more sophisticated militarily and ideologically.

A major oversight of the study is lack of individual or collective biography. For example the ques-

tionable theory that the *tenentes* were middle-class reformers is repeated without new proof. The police records should have afforded easily quantifiable data. Moreover, thumbnail sketches of the major actors would have added life to the narrative; yet even the legendary João Cabanas receives only a footnote. In terms of format, excessive and lengthy footnotes often account for more than half the printed page. Otherwise, though, the book meets the high standards of the Brazilian social science press.

Corrêa's insistence on relating the events of São Paulo to class structure adds depth to this study and reflects the professionalism of São Paulo's historians. Such efforts will lead away from hypothetical social models toward greater understanding of pre-1930 society in general.

MICHAEL L. CONNIFF
University of New Mexico

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

PAUL USELDING, editor. *Research in Economic History: An Annual Compilation of Research*. Volume 2, 1977. Greenwich, Conn.: JAI Press. 1977. Pp. viii, 389. \$12.50.

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Other Books Received

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GENERAL

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Recent Deaths

WOOD GRAY, professor emeritus of American history at George Washington University, died at his home in Georgetown on June 27, 1977. For more than twenty years Professor Gray had served the *American Historical Review* as editor of the United States history section of "Recently Published Articles." Otherwise, he was best known to colleagues as the author of the *Historian's Handbook*, a professional guide to the study and writing of history used by students of history at more than 500 colleges and universities. At the time of his death, Professor Gray had been working daily on revisions for a new edition of the *Handbook*. Besides this work, his principal publication was a history of the Copperheads, entitled *The Hidden Civil War*, first published in 1942 and subsequently brought out in paperback in 1964.

Except for three years he spent with the U.S. Air Force during World War II, Professor Gray had taught at George Washington University since 1934. He was chairman of the history department from 1937 to 1953. Always concerned for faculty rights, he led a successful movement within the university for the adoption of one of the nation's most liberal faculty codes. And in keeping with that concern, he was a prime mover in the establishment of a university senate. During a recent meeting of that body, a colleague offered a memorial statement which described him as follows: "As a teacher, Wood Gray imparted information with every breath he drew. His range of information, which he interpreted simultaneously with rare humor and insight, brought an intellectual feast to his students. It was, however, Wood Gray's ardent advocacy of faculty causes that keeps him alive in our memories. Almost singlehandedly he brought an AAUP chapter to this campus. And with a small band of determined colleagues, he helped to originate this assembly in which we sit today: the Faculty Senate. Throughout his career at George Washington, no effort to enhance the prestige and influence of the faculty transpired without his leadership. No administrator ever intimidated him, although some tried. And no opposition was too formidable to deter him whenever he was

locked in combat on behalf of the faculty." Fellow Washingtonians also recall that as a member of the admissions committee of the Cosmos Club, Professor Gray helped to integrate the club with the admission of fellow historian John Hope Franklin.

Born in Petersburg, Illinois, 72 years ago, he received a bachelor's degree from the University of Illinois in 1927 and completed his doctoral work at the University of Chicago where he also taught in 1933. His department held a memorial service for Wood Gray on June 30.

PETER P. HILL

George Washington University

WILLIAM L. LANGER, who died on 26 December, 1977, at the age of eighty-one, embodied in his life and work all the dimensions of the historian's vocation. He broke through the barriers which have increasingly separated the professional community from the wider public, the scholar from the student, the academy from the public realm. Langer fittingly entitled his autobiography (its advance copies reached him only a few days before his death), *In and Out of the Ivory Tower*. Uniting the "in" with the "out," he kindled a kind of light by which past and present, scholarship and politics could illuminate each other.

The second of three sons of an early-widowed German mother, Langer grew up in South Boston under conditions of penury and asceticism that can only be called Dickensian. Rare were the sunbursts of fortune that broke through the pall of deprivation—such as being sent by his grammar school to Boston Latin School, whence he could enter Harvard College as a struggling, lonely day-scholar. Early experience gave Langer an earthy sense of reality in all its inchoate and insistent mystery: delight in its variety, respect for its power, alertness to its unexpected transformations, and a strong empathy for men trying to cope with it.

World War I changed Langer's life from hardship and frustration to action and affirmation. With the innocent enthusiasm so common to his

generation, he enlisted, leaving behind a cramping career as language teacher. The great issues of war and peace turned him to history as the center of his intellectual life.

Even before he entered graduate school at Harvard in 1919, Langer published a fledgling work of history: *With "E" of the First Gas*. Introducing a reissue of this book (under the title, *Gas and Flame in World War I* [1965]), Langer recreated with charming self-irony the naive cultural climate in which, as actor and as author, he had produced it. The sobering experience of the defeat of Wilsonian hopes in the early twenties, however, dominated the atmosphere in which Langer was introduced to modern European history. His two principal graduate teachers, Archibald Coolidge and Robert Lord, participants in Wilson's "Inquiry" preparing for the United States role in peace making, served young Langer as role models not only as scholars, but also as historian-experts in government service. As the first editor of *Foreign Affairs*, Coolidge also enlisted Langer as compiler of its annotated bibliography. Out of this grew a life-long association with the Council on Foreign Relations, the most important forum for the exploration of international questions among the business, academic, and governmental elites. Scholarship, government service, and public participation in political opinion formation thus appeared early as related constituents of this modern historian's vocation.

In an atmosphere charged with debates on Versailles, the League, and responsibility for the European War, diplomatic history inevitably drew many of the most vital young minds in the profession in the twenties. Most who studied European international relations before 1914, however, became involved in the more immediate origins of the war and in the war-guilt question. Langer took his own road. In the introduction to his path-breaking *European Alliances and Alignments* he wrote in 1931, "I have written this book not with the outbreak of the World War especially in mind, but as a study in the evolution of the European states system." If Bismarck, the "master-builder of the Wilhelmstrasse," stood at the center of the story, Langer developed out of it a *systematic* understanding of national power-politics. The combination of magisterial command of the sources and clarity of analysis shown in his work catapulted Langer to international recognition overnight.

Having charted the construction of the post-1870 states system, Langer turned to its dissolution in *The Diplomacy of Imperialism* (1935). He showed the grave *allemande* of the Bismarckian era accelerating through increasingly eccentric criss-crosses of imperialist powerpartnerships into a political *danse macabre*. But why? Even as he achieved his lumi-

nous structural clarification of the European international system, bringing Rankean methods to new heights of analytic and synoptic sophistication, Langer developed doubts about the sufficiency of diplomatic history and its autonomy as a field. What had happened to the international game that it destroyed its players? One could not answer the question without anchoring international history in its wider social and cultural matrix. Even within *The Diplomacy of Imperialism*, Langer inserted a chapter opening in his inimitably concrete way some theoretical questions about the dynamics of imperialism, using British society as a demonstration ground. Though this chapter seems somewhat superficial today, those who were students of history in the mid 1930s will remember the tremendous excitement caused by the path-breaking "Chapter 3." It was the first of many instances in which Langer, from the platform of his own achievement as master-consolidator of a tradition, pointed beyond his own work to new questions and to the new forms of historiography necessary to resolve them.

At Clark University, 1923-27, and thereafter at Harvard, Langer pursued in his teaching those more universal interests that could not be contained in diplomatic history. In his undergraduate lecture course on Europe since 1815, he developed a richly textured comparative analysis of European national societies in their dynamic interaction. He brought a similar synoptic vision to bear on his influential series, *The Rise of Modern Europe*. In an era when both the erosion of political narrative and the hypertrophe of professional specialization threatened in their separate ways the very idea of general history, Langer enlisted America's best scholars to write the history of Europe in synchronic cross-sections. As an imaginative editor he thus reinvigorated the tradition of European universal history by defining a new scale of time and social space which made possible a textural treatment of the various constituents of culture, not just politics. Though the success of the volumes varied, they were as educational for the authors as for the reading public. "The Langer series" both broadened the frame of history teaching and contributed to breaking the grip of the textbook.

Langer's realism produced in his politics a deeply conservative outlook. At the same time, his very perception of reality as many-faceted and protean convinced him that many modes of thought were needed to comprehend it—whence the remarkable openness and ecumenism that marked his teaching and his thinking. As graduate instructor, he prided himself on producing no disciples in the German sense, who faithfully followed the master's ways, but scholars who worked in a vast

variety of fields with an almost equal variety of methods on problems of their own choosing. It is probably true to say that he regarded as his students rather those who took his rigorous seminar, into which he poured heart and mind, than the narrower group who wrote theses under him. The latter he regarded as too mature at that stage to be interfered with.

In World War II, Langer entered government service, revealing previously untapped talents as a scholarly administrator. In organizing the Research and Analysis Branch of the Office of Strategic Services, he deliberately recruited scholars young and old of the most diverse intellectual and social outlooks, from the most radical to the most conservative. Thus the ecumenism of his scholarship and teaching extended to his exercise of administrative responsibilities. However great his concern for the national interest as officially defined—and it was deep—he never shared in the spirit of paranoiac contraction that began to invade our government as World War II drew to a close.

Langer's years of service in the OSS (1941–46), the Department of State (1946), and the Central Intelligence Agency (1950–51) rekindled his interest in diplomatic history, this time of a contemporary character. In an amazingly short time, Langer produced three thorough if sometimes controversial works on the development of American foreign policy in its confrontation with the Axis (*Our Vichy Gamble* [1947]; with Everett S. Gleason, *The Challenge to Isolation 1937–1940* [1952]; and *The Undeclared War* [1953]). Only after their publication did he return to the work that engaged his interest over twenty-five years: *Political and Social Upheaval, 1832–1852* (1969), a crowning volume for his own series.

His retirement in 1964 brought no diminution in Langer's intellectual vitality. Serious bouts of illness could slow his almost superhuman capacity for work, but never break it. Sustained by an insatiable appetite for exploring the hidden springs of historical change, he did much of his most original work in the final two decades of his life. Most productive historians devote their last years to consolidation. Langer had long passed this phase, producing his masterful syntheses by the time he was forty. He devoted his final years to the intellectual explorations that one associates with youth, opening one door after another into the historiographical future. As he had probed intellectual history in the 1930s for its yield in understanding modern Nazism, so now he explored in pioneering articles the use of psychoanalysis in history, epidemiological history, demographic history, and so on. (See for example, *Explorations in Crisis* [1969], especially the papers in Part 4, "Ex-

ploration in New Terrain," and "Checks on Population Growth, 1750–1850," *Scientific American* 226 [February 1972]: 92–99.) Surely Langer's was one of the freshest, most vigorous, and most capacious spirits to arise in the American historical profession.

CARL E. SCHORSKE
Princeton University

FREDERICK MERK, Gurney Professor of History and Political Science Emeritus and for almost forty years a beloved and inspiring teacher of Harvard undergraduate and graduate students, died of a heart attack in Cambridge on September 24, 1977. Less than six weeks earlier, in good health and spirits, he had celebrated his ninetieth birthday at his son's summer cottage where, in his words, "twenty members of the clan gathered . . . and a good time was had by all!" In a letter to this writer on September 1, he concluded: "I am at the moment waiting galley proof for my study of the History of the Westward Movement."

Born in Milwaukee on August 15, 1887, Merk attended local schools and the University of Wisconsin, receiving his bachelor's degree in 1911. His plan to study under Frederick Jackson Turner in his senior year was frustrated when Turner moved to Harvard in 1910. Lacking funds for graduate work, Merk spent the next five years on the editorial staff of the State Historical Society of Wisconsin during which time he collaborated with Reuben Gold Thwaites and Asa Currier Tilton in editing in 1912 the *Civil War Messages and Proclamations of Wisconsin War Governors*. In 1916 he published his first book, *Economic History of Wisconsin during the Civil War Decade*, which remains the standard treatment.

In 1916, Merk followed Turner to Harvard and, with fellowship aid, began his doctoral studies. In 1918, he became a tutor in the Division of History, Government, and Economics. During his two-year tenure he completed the requirements for the doctorate, winning the Toppan Prize for a dissertation that was, in effect, his first book. The year 1920–1921 was spent on a Sheldon Traveling Fellowship, mostly in England, examining British materials on the Oregon question, a subject he had first investigated in Turner's seminar and one that would engage him for years to come. He returned to Harvard in 1921 as an instructor, and was promoted to assistant professor in 1924, associate professor in 1930, and professor in 1936. After serving as departmental chairman during the difficult war period, 1941 to 1946, he held the Gurney chair from 1946 to his retirement in 1957. During the academic year 1927–1928, under an experimental exchange program, he taught at Carleton, Grinnell, and Pomona.

Even as an instructor, Merk played a key role in the offerings in United States history. From 1921 to 1926, he shared with Charles H. McIlwain a two-semester course in American constitutional history. In 1922, he took over the first half of the American survey; the second was given by Edward Channing and then, starting in 1924, by Arthur M. Schlesinger. Later Paul H. Buck and Frank Freidel participated. But Merk's best known course, through which he influenced countless future scholars, was the History of the Westward Movement. For three years beginning in 1921, Turner taught the first semester and Merk the second. When Turner retired in 1924, Merk took sole charge and offered it almost every year until 1957. For more than two decades virtually every graduate student in United States history enrolled in his research seminar. Some fifty went on to prepare dissertations under his direction, while others, including the undersigned, repeatedly turned to him for advice and assistance in their teaching, writing, and professional activity.

After his early publications on the Civil War era, Merk focused on the period 1815 to 1850, especially the Oregon question; but except for *Fur Trade and Empire: George Simpson's Journal, 1824-1825*, which he edited in 1931, he confined himself initially to articles. Lucid in style and exhaustive in research, each of these gem-like essays contributed a new and significant interpretation. Beginning with "The Oregon Pioneers and the Boundary" in July 1924, he followed with "British Party Politics and the Oregon Treaty" in July 1932, "Snake Country Expedition, 1824-25" in June 1934, "The British Corn Crisis of 1845-46 and the Oregon Treaty" in July 1934, "British Government Propaganda and the Oregon Treaty" in October 1934, "The Genesis of the Oregon Question" in March 1950, "The Ghost River Caledonia in the Oregon Negotiation of 1818" in April 1950, and "The Oregon Question in the Webster-Ashburton Negotiations" in December 1956. Four of these appeared in the *American Historical Review*, three in the *Mississippi Valley Historical Review*, and one in *Agricultural History*. Late in 1950, he published a 97-page monograph on the negotiations of 1826-1827 under the title *Albert Gallatin and the Oregon Problem*. To this list should be added "Eastern Antecedents of the Granger Movement," printed in *Agricultural History* in January 1949 and his substantial contribution to the *Harvard Guide to American History* published in 1954. In this last he was particularly interested in the sections dealing with maps, bibliographies and other aids to research, and the period 1789 to 1829.

Retirement ushered in twenty years of great productivity. First came two more articles in the *Mississippi Valley Historical Review*—"Presidential FEVERS" in June 1960 and "A Safety Valve Thesis and

Texan Annexation" in December 1962. Then, with the increasing collaboration of his wife and former student, Lois Bannister, he published in rapid order *Manifest Destiny and Mission in American History: A Reinterpretation* in 1963; *The Monroe Doctrine and American Expansionism, 1843-1849* in 1966; *The Oregon Question: Essays in Anglo-American Diplomacy and Politics* in 1967, a volume containing nine articles previously printed, the Gallatin monograph, four unpublished essays, and a résumé or overview; *Fruits of Propaganda in the Tyler Administration* in 1971; and *Slavery and the Annexation of Texas* in 1972. Slightly revised editions of *Fur Trade and Empire* and of *Economic History of Wisconsin during the Civil War Decade* were issued in 1968 and 1971. A lecture on dissent in the Mexican War, which also appeared in the *Massachusetts Historical Society Proceedings*, was included in 1970 in *Dissent in Three American Wars* with Samuel Eliot Morison and Frank Freidel as the other authors. By 1972 he had yielded to the urgings of his friend of many years, Alfred A. Knopf, to put his Harvard lectures into print. The *History of the Westward Movement*, in galley proof when death struck, will be released in 1978.

A longtime member of the American Historical Association—he attended the Chicago meeting in 1912—Merk served on the Membership Committee from 1924 to 1926, the Justin Winsor Prize Committee from 1927 to 1929, the Nominations Committee from 1929 to 1931, and the Program Committee in 1932-1933. Elected to the Council in 1935 for a four-year term, he was active on its Executive and Appointments Committees. He was president of the Mississippi Valley Historical Association in 1959-1960 and sat on the Executive Board for six years thereafter. In 1948 he was president of the Agricultural History Society. He was a Fellow of the American Academy of Arts and Sciences and a member of the American Antiquarian Society and the Massachusetts Historical Society. He is survived by his wife, two children, and four grandchildren.

Merk was a superb teacher at all levels, not only because of his mastery of the subject but also because of his obvious enthusiasm and utter sincerity. As a scholar, he was a perfectionist in the correct sense of the word, unwilling to publish prematurely for profit or promotion and unable to produce anything except the very best. As to his reasons for studying history, he once told a graduate class that he placed first the opportunity for self-expression. At the lectern, in the seminar room, and on the printed page, he did indeed reveal himself—a man of courage, dedication, compassion, wisdom, and, above all, integrity.

RICHARD W. LEOPOLD
Northwestern University

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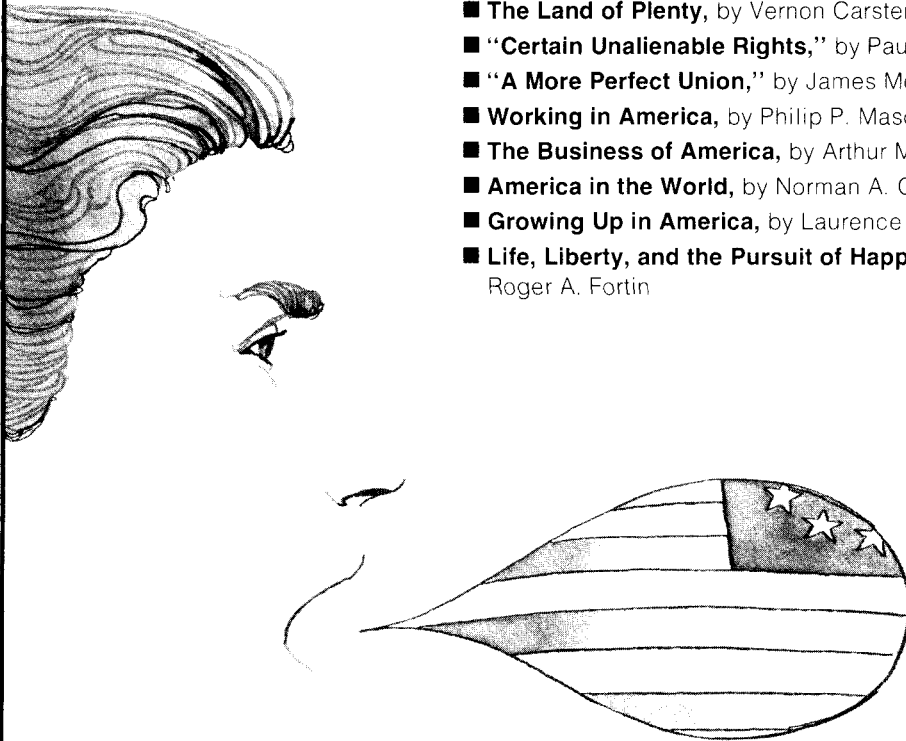
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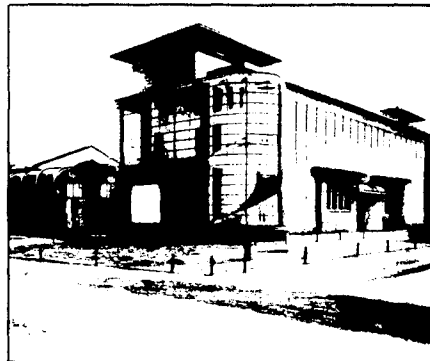
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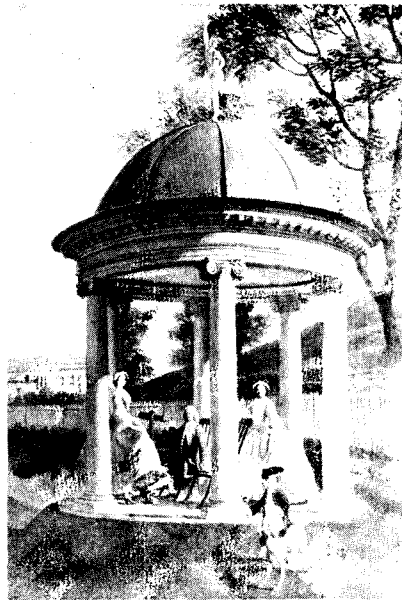
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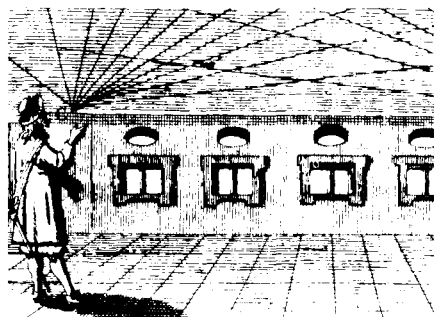
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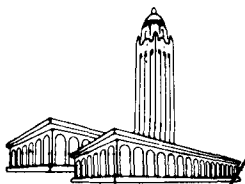
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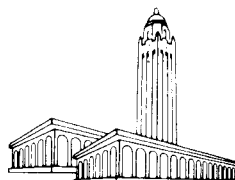
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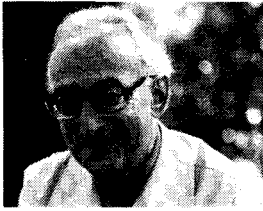
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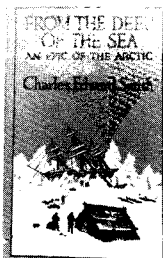
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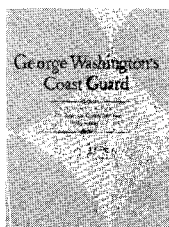


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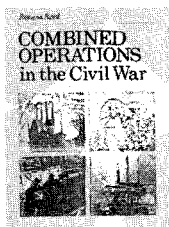
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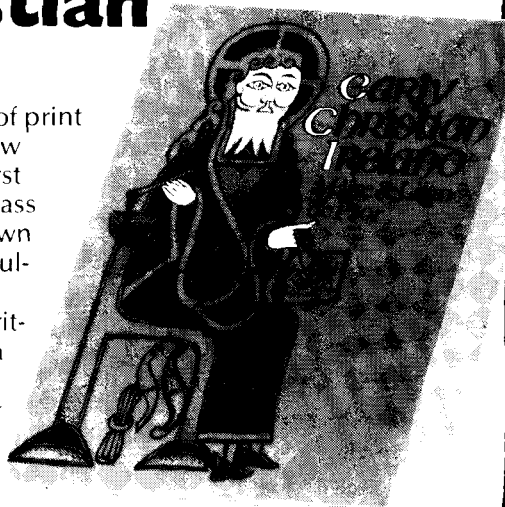
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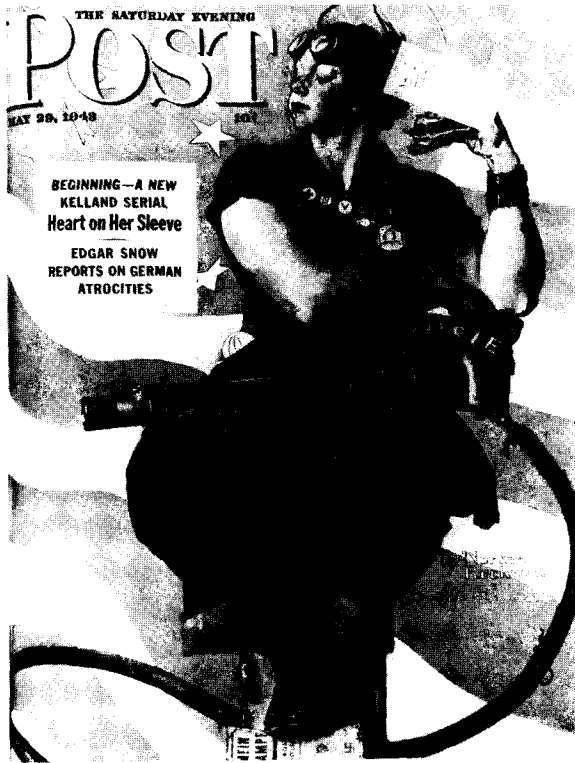
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